Disparate Wages in a Globalized World

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Abstract

This study uses wage data from the UBS Prices and Earnings survey to highlight *Disparate Wages in a Globalized World* from different perspectives. This wage data is characterised by remarkable consistency over the last 40 years, as well as unusual global comparability.

In the first chapter we analyse the convergence hypothesis for purchasing power adjusted wages across the world for 1970 to 2009. The results provide solid evidence for the hypotheses of absolute and conditional convergence in real wages, with the key driver being faster overall growing wage levels in lower wage countries compared to higher wage countries. At the same time, the highest skilled professions have experienced the highest wage growth, while low skilled workers' wages have lagged, thus no convergence in this sense is found between skill groups.

In the second chapter we examine deviations in international wages from Factor Price Equalisation theory (FPE). Following an approach analogous to Engel (1993) we find that deviations from FPE are more likely driven by the higher variability of wages between countries than by the variability of different wages within countries. With regard to the traditional analysis of the real exchange rate and the Balassa-Samuelson assumptions our analysis points to a larger impact on the real exchange rate likely stemming from the movements in the real exchange rate of tradables, and only to a lesser extent from the lack of equalisation of wages within countries.

In the third chapter our results show that India's economic and trade liberalisation, starting in the early 1990s, had very differential impacts on skill premia, both over time and over skill levels. The most striking result is the large increase in wage inequality of high-skilled versus low-skilled professions. Both the synthetic control group method and the difference-in-differences (DID) approach suggest that a significant part of this increase in wage inequality can be attributed to India's liberalisation.

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Abbreviations

AIESEC	International Association of Students in Economic and Commercial Sciences
ANOVA	Analysis of Variance
AR	Auto Regressive
BLS	US Bureau of Labor Statistics
cagr	Compound annual growth rate
CIS	Commonwealth of Independent States
CPI	Consumer Price Index
DID	Difference-in-Differences
EU	European Union
FDI	Foreign Direct Investment
FGLS	Feasible Generalised Least Squares
FPC	Factor Price Convergence
FPE	Factor Price Equalisation
GATT	General Agreement on Tariffs and Trade
GDP	Gross Domestic Product
GNI	Gross National Income
H60	Higher education in 1960
HDI	Human Development Index

HDRO	Human Development Report Office
ICP	International Comparison Programm
IMF	International Monetary Fund
MAD	Median Absolute Deviation
mpl	Marginal product of labour
NA	Not applicable; no data available
NAFTA	North American Free Trade Agreement
NSS	National Sample Survey of India
OECD	Organisation for Economic Cooperation and Development
OLS	Ordinary Least Squares
OWW	Occupational Wages around the World
PC	Principal Component
PCA	Principal Components Analysis
PCAR	Principal Components Augmented Regressions
PPP	Purchasing Power Parity
PWT	Penn World Tables
R^2	Coefficient of determination R-squared
RER	Real exchange rate
SBTC	Skill-biased technological change
SDM	Sala-i-Martin, Doppelhofer and Miller
SUR	Seemingly Unrelated Regressions
UBS	Swiss Bank UBS which conducts the UBS Prices and Earnings survey
UNDESA	United Nations Department of Economic and Social Affairs
UNESCO	United Nations Educational, Scientific and Cultural Organisation

US	United States
USD	United States Dollar
WDI	World Development Indicators
WTO	World Trade Organisation

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Preface

"Only in our dreams are we free. The rest of the time we need wages."
Terry Pratchett

Real wages define living standards for a large share of the world's population. Substantial differences in real wages within a society can act as an incentive to achieve higher levels of education and skills – at extremes, however, large wage differentials can also be a major strain to the social fabric and to social and political stability. At an international level differences in wages can impact countries' competitiveness, they are a major factor when determining commercial strategies such as outsourcing and production sharing, and they are the key driver of companies' assessments of the potential of future consumer markets. Wage differentials will thus often be a driver of capital and investment flows. Additionally, workers react to real wage incentives, and large differentials between countries can be a trigger for legal and illegal immigration. In political, social and economic terms, wages are a lynchpin of any society.

Given their importance, wages have received extensive attention in economic theory, from micro- and macro-economic perspectives, and in closed and open economy models. The most prominent theories describing wage determination include the neoclassical model of labour-leisure choice combined with marginal productivity theory, the theories of industry wage premia, unionisation, efficiency wages and compensating wage differentials, the Balassa-Samuelson effect, migration theory, trade theory and in particular the Heckscher-Ohlin framework of trade with the Factor Price Equalisation theory (FPE) and Skill-Biased Technological Change (SBTC), and theories of government policy including minimum wages, social security, tariffs, quotas and non-tariff barriers.

While most of these theories have been tested extensively using data for individual countries, or comparisons for a small number of countries, a dearth of comparable wage data across a large number of countries makes wider international comparisons scarce. In particular, studies requiring comparable data for sectors, or for different levels of skills across a wide range of countries, face substantial data limitations. This study uses a dataset that has not been substantially exploited in the past to analyse wages and skill premia in an international context.

The wage data is drawn from the UBS Prices and Earnings survey, which was conducted in 31 cities around the world in 1970, rising to 71 cities in 2009, in three year intervals. The key strength of the UBS data is its remarkable consistency over the last 40 years, as well as its unusual global comparability, as in each country the survey was conducted with an identical questionnaire and comparable methods during the same time interval. Appendix A provides detailed information on the UBS Prices and Earnings survey and the wage data used in the following chapters. Each chapter of this dissertation highlights *Disparate Wages in a Globalized World* from a different perspective.

In the first chapter the UBS wage data is used to analyse the convergence hypothesis for purchasing power adjusted wages across the world for the period 1970 to 2009. We derive the theoretical basis for wage convergence within the Solow-Swan model of economic growth, but show that wage convergence can be viewed also within the contexts of trade theory, and migration theory.

We find no clear statistical evidence of an absolute catch-up in wages for poorer countries as measured by GDP per capita, but we find solid evidence of absolute convergence in two senses: Cities with lower *average* initial real wages exhibit higher growth of *average wages*; and lower initial *profession level* real wages world wide (whether due to the country context or the profession) also exhibit higher growth. Convergence in these senses is stronger in the latter half of our observation period 1988-2009, but absolute convergence holds also for the complete period 1970-2009. Evidence of absolute convergence is even stronger when the sample is restricted to more homogenous groups of countries by subdividing the sample into developed and emerging markets. Convergence is also stronger within more homogenous skill groups: It is strongest within individual professions across the world, somewhat less strong within professions clustered according to their skill levels, and least strong when all professions are included.

We test for conditional convergence in the Solow-Swan sense by including variables that control for the steady state in the extended Solow-Swan model. Not all of the included variables are statistically significant, but including the initial wage level, the fertility rate and an education measure as regressors provides a good model fit. The results provide some support to the Solow-Swan model given the relatively high coefficients of determination of around 30-70% achieved with these simple regressions, as compared to 20-30% for the absolute convergence regressions. However, given the lack of evidence of any impact of investment in physical capital and the very limited evidence for the role of investment in human capital, this evidence remains ambiguous. With regard to the speed of convergence, the coefficients on the initial wage point to a convergence speed σ_w of 1.0% to 1.7% per annum, somewhat faster than the unconditional rate of convergence for the full sample and equivalent time period of about 1.0% per annum.

We aim to provide more granularity on the question of conditional convergence, by investigating who the winners and losers of the convergence trends have been. Compared to low-skilled professions, we find higher average wage growth for medium skilled, and even higher wage growth for highly skilled professions. This finding can be interpreted as evidence for skill-biased technological change that has resulted in a higher demand for skilled labour relative to unskilled labour, thereby increasing wage rates for the skilled faster, than for the unskilled. This result does not contradict the finding of convergence described above, but rather gives insight into how convergence came about. Wage convergence across the world was *not* based on lower skilled professions gaining on higher skilled professions. Rather, the primary driver of international wage convergence was faster overall growing wage levels in lower wage countries, as compared to higher wage countries.

Finally, we use principal component augmented regressions to analyse the impact of a broader range of variables on real wage growth. The initial wage and the fertility rate are confirmed as consistently having a large and statistically significant impact on wage growth. Cities in Latin America experienced statistically significant lower levels of wage growth and skill level dummy variables point to higher wage growth in professions with higher skill levels. Additionally, the fraction of the population speaking English was found to have a large, positive and statistically significant impact on wage growth.

Overall these results support the hypotheses of absolute and conditional convergence in real wages, with the key driver being faster overall growing wage levels in lower wage countries, as compared to higher wage countries. At the same time, the highest skilled professions around the world have experienced the highest wage growth during 1970-2009, while low skilled workers experienced the lowest wage growth, thus no convergence in this sense is found between skill groups. This more differentiated understanding of convergence trends in real wages provides important food for thought in a world where inequality is increasingly acting as a driver for social discord and unrest.

Thus, convergence in global wages – in the absolute and in the conditional sense of Solow-Swan – is a strong and robust result. However, in spite of convergence, large disparities in international wages persist. In the second chapter the UBS wage data is employed to examine deviations from Factor Price Equalisation theory (FPE). FPE theory asserts that under certain conditions trade in goods can act as a substitute for factor mobility, implying that wages will converge even without factor flows between countries. However, empirical evidence is mixed and FPE theory seems at odds with large and persistent divergences in international wages.

This chapter contributes to explaining the origins of deviations from FPE by drawing a parallel to the analogous "price" problem - i.e. the origin of deviations from Purchasing Power Parity (PPP). As PPP would imply a constant real exchange rate of one, fluctuations in the real exchange rate reflect deviations from PPP theory. In 1993 Charles Engel set an important empirical yardstick for models of the real exchange rate by decomposing the expression for the real exchange rate and analysing the variability of the parts (i.e. variability of relative consumer prices within and between countries). By identifying the origin of volatility in the real exchange rate, Engel (1993) pinpoints the origin of the deviations from PPP.

In the second chapter the equivalent relationship between the real exchange rate and relative wages is investigated, by decomposing an adapted expression for the real exchange rate, now based on factor prices (wages), instead of goods prices. In fact, the theory of FPE can be viewed as the production side analog to PPP on the consumption side. As FPE would imply a constant wage-based real exchange rate of one, we analogously to Engel (1993) investigate whether deviations from FPE have historically been better explained by the variability in the wages between countries, or by the variability of wages within countries, thereby providing an empirical yardstick for models of factor prices.

We use a number of comparable wage indices calculated from the UBS Prices and Earnings wage data and three different measures for variability (scale parameters), as well as profession level wage data corrected for fixed effects to measure variability in wages. Our results show that historically the variability of identical wage indices between countries has been higher than the variability of different wage indices within a country. This result is analogous to Engel's early result, and indicates that deviations from FPE are more likely driven by the higher variability of wages between countries, than by the variability of different wages within countries. Interestingly, there is no clear evidence of structural differences in the wage comparisons involving only developed markets and those involving also wages in emerging markets.

With regards to the traditional analysis of the real exchange rate this analysis is informative in that it shows that the Balassa-Samuelson assumptions of a constant real exchange rate in the tradables sector, and of wage equalisation between the tradables and non-tradables sectors have a differing importance with regard to movements in the real exchange rate. While the literature mostly confirms that these assumptions do not hold empirically, our analysis points to a larger impact on the real exchange rate likely stemming from the movements in the real exchange rate of tradables, and only to a lesser extent from the lack of equalisation of wages within countries. Broadly speaking this analysis is in line with the Balassa-Samuelson projections in that it finds more homogeneity of wages within a country, than between countries. It emphasises that models of international factor prices explaining deviations from FPE should encompass explanations for the large differences and variability in wages of equivalent workers in equivalent jobs across different countries, and offer explanations of much smaller variability in differences between professions' wages within a country.

In the third chapter, which is based on Boes and Weisser (2012), the UBS Prices and Earnings data is used to provide empirical evidence of the medium- and long-term consequences of India's economic liberalisation from the early 1990s on skill premia. As urban and rural areas are affected very differently by trade opening and liberalisation, we focus on urban wage inequality and the city of Mumbai, an important trading hub, in this chapter. We address the identification problem of the unobserved counterfactual outcome by employing the synthetic control group methodology to construct wage trends for the city of Mumbai under the counterfactual scenario of no reforms. We use a broad group of 35 other large cities, including many trading centres in emerging markets, to construct the synthetic control.

We find that in the first phase after the 1991 liberalisation medium-skilled wages fell relative to high- and low-skilled wages. Our synthetic control group shows that these losses were stronger than would have been expected if no reforms had been implemented. This implies a reduction in medium- to low-skilled wage inequality, but an increase in high- to medium-skilled wage inequality.

From the late 1990s onwards, the growth of high-skilled wages outpaced the wages of the other two skill groups, pointing to new forces of globalisation kicking in after the initial reforms. We find some evidence that in a third phase, from the second half of the 2000s, medium-skilled wages accelerated and by 2009 had compensated for the previous losses relative to high-skilled wages. Versus the low-skilled, medium-skilled workers were even able to overcompensate earlier losses relative to what would have been expected without treatment.

Overall, our results in this final chapter allow for two main conclusions. First, India's economic and trade liberalisation, starting in the early 1990s, had very differential impacts on skill premia, both over time and over skill levels. The most striking result is the large increase in wage inequality of high-skilled versus low-skilled professions. Second, the results from both the synthetic control group method and the difference-in-differences (DID) approach suggest that a significant part of this increase in wage inequality can be attributed to India's liberalisation. While DID provides rather uninformative results, the synthetic control group methodology suggests that this overall rise in inequality was not a one-way process because we find some periods in which measures of wage inequality were stable, or even falling.

Finally, our analysis points to a possible explanation of the diverging results in the literature. While most studies distinguish only between skilled and unskilled workers, and group together a large number of different sectors, our results suggest that different levels of skill, such as in our case low-, medium- and high-skilled workers were likely impacted very differently. Therefore, a more disaggregated analysis seems likely to be more informative than the general definitions often used in the literature to define the skill premia.

Chapter 1

A Contribution to the Empirics of International Wage Convergence

1.1 Introduction

The goal of understanding why some countries flourish, while others linger in poverty has long been a key motivational force for the economics profession. The hope of contributing meaningful policy advice that will enable countries to improve their population's well-being might not yet have been Adam Smith's explicit motive in 1776 for writing on the "nature and causes of the wealth of nations", but it has likely been a prominent factor for a large share of economists since. The insight of the powerful effect of compound interest – that even small differences in annual growth rates result in large differences in countries' well-being over time – has focused economists' attention firmly on the determinants of economic growth.

Today there is no single framework that unifies all the diverse perspectives under which growth – and thus the improvements of countries' well-being – have been analysed. Many key elements in contemporary theory stem from the early classical economists: Adam Smith himself contributed significantly to the understanding of the role of competitive behaviour in fostering higher productivity, and growth. He also highlighted the role of human capital – a mainstay in modern growth theories – through the refining of workers' skills in the process of labour specialisation and recognised that human capital and technological change are closely related. Later economists explicitly modelled technological progress as a function of innovation. Schumpeter (1942), for example, argued that competitive behaviour and the profit motive could justify investments in human capital and that the innovations resulting from R&D are the fundamental drivers for economic growth.

David Ricardo (1817) was one of the first to describe two further key elements of modern growth theory – capital accumulation and diminishing returns. Ricardo recognised that more and improved machinery increased labour productivity and therefore a country's productive capacity. Ricardo was doubtful whether technical change could counteract the diminishing marginal returns that he observed in agriculture – a feature of production widely integrated in parts of today's growth literature. However, while Ricardo attributed diminishing returns to a decreasing quality of inputs, this characteristic is today attributed rather to the increasing production factor's reduced per unit endowment of the other fixed production factors.

Thomas Malthus in 1798 was likely the first to formalise a view on another key element affecting well-being. Malthus believed that population growth, i.e. fertility and mortality, would adjust up or down until all individuals reached a subsistence level of consumption. While this very negative view has been disproved by the emergence of wealthy societies, the persistence of extreme poverty in parts of the world has lead some economists nonetheless to integrate Malthus's views into contemporary thinking. For example, Galor and Weil (2000) formulate a model based on Gary Becker's (1960) quantity-quality-hypothesis in which a "Malthusian regime" can exist in which skill is not productive enough to warrant investments in children's education leading to a poverty trap with high population growth. However, in "demographic transition" low unskilled wages provide an incentive to invest in children's skills (quality) rather than their number (quantity), so that population growth rates decline and average well-being starts to rise. Malthus's ideas on population growth also shaped later economic models in which population growth was sometimes seen as endogenous, i.e. as being the consequence, rather than the cause of a specific socio-economic environment.

While the classical economists provided much of the groundwork, the cornerstone of today's economic thinking on growth was laid in the neoclassical model developed by Robert Solow (1956) and Trevor Swan (1956). Based on the work of Harrod (1939) and Domar (1946), it incorporated capital accumulation, population growth and productivity growth as exogenous inputs in a neoclassical production function with constant returns to scale and diminishing returns to factor inputs. The tremendous success of this approach, in particular its augmented version which further includes human capital accumulation (cf. Mankiw et al., 1992), stems from its effective merging of the most widely understood drivers of economic growth to generate a very simple and intuitive general equilibrium model: Per capita income in the long-run steady state will be higher for countries with higher rates of savings in physical and human capital, and for lower rates of population growth. However, once the steady state has been reached, the growth of per capita output depends only on the rate of technological progress. A fundamental criticism of the Solow-Swan model is precisely this – both the level of steady state per capita income and per capita income growth in the steady state depend wholly on *exogenous* variables, whose levels the models cannot explain. Additionally, empirical patterns suggesting that the savings rate on average rises as countries get richer, and that fertility tends to decline, stand in contrast to the Solow-Swan model's assumption of constant savings and population growth rates (cf. Barro and Sala-i-Martin, 2004, p. 16). This unsatisfactory state gave rise to the development of *endogenous* growth models from the mid-1980s, whose ambition was to determine the long run growth rate based on factors that are determined within the model. Nonetheless, the Solow-Swan model has established itself as the primary reference point in growth economics and its success remains formidable, in part also due to its well-known prediction of "conditional convergence". This hypothesis implies that the further away a country is from its steady state, the higher the growth rates of per capita income will be as diminishing returns to capital slow growth as a country approaches its steady state. Thus, conditional convergence implies that growth rates between structurally similar countries can differ significantly based solely on differences in the distance from a shared equilibrium point, an important factor when interpreting countries' relative growth performance. The implication is that poorer countries would have a tendency to catch up to richer ones, provided they share the same steady state. While many of the endogenous growth models do not share the convergence characteristic, some prominent examples do, e.g. the model of Ramsey-Cass-Koopmans in which the savings rate is endogenously determined through intertemporal consumer optimisation, as well as models of technology diffusion in which poorer countries can imitate the technological advances of wealthier countries.

While conditional convergence as measured by a country's average per capita income has mostly established itself as an empirical regularity in the literature, giving strong support to the Solow-Swan model, convergence by no means provides economists with an all-encompassing explanation in the understanding of economic well-being. The main criticism of this approach is its highly aggregated nature. Comparing aggregate gross domestic product (GDP) per capita between countries glosses over significant distributional effects: Is it capital or labour that is benefitting more from an average increase in income per capita? Are highly skilled workers and low-skilled workers within a country affected similarly by convergence? Is income per capita the most appropriate means to measure economic well-being? Williamson (1996, p. 300) goes as far as to ask, whether we even know for sure whether convergence is a good thing. He asserts, "Convergence of what? The new growth theory has shown very little interest in who gains and who loses from convergence. The theory tends to be highly aggregative, and its empirical applications deal with coarse aggregates like gross domestic product per worker. What about wages of unskilled laborers, wages of skilled artisans, salaries of skilled clerical workers, farm rents accruing to landlords, and profits accruing to capitalists? What about returns to sector-specific resources and capital?"

This chapter aims to address some of these issues by providing empirical evidence on convergence over the past 40 years in a more disaggregated fashion. First, with the goal of reaching a more distinct interpretation of economic well-being, we focus on the question of convergence in purchasing power adjusted (real) wages, rather than in aggregate per capita income, thereby removing from the equation the question of changes in capital versus labour's share of income. Second, we provide some disaggregation with regard to workers' skill levels and professions to identify differing impacts of convergence on societal groups with the aim of better identifying the winners and losers of the great convergence debate.

The structure of this chapter is as follows: Section 1.2 reviews the theoretical background of the convergence argument in the Solow-Swan growth model and derives an equivalent justification for convergence in wages. Section 1.3 describes other potential drivers of wage convergence while section 1.4 reviews the empirical literature on growth and wage convergence. The data used for the wage convergence analysis is described in section 1.5 (and in more detail in appendix A) and section 1.6 displays some key trends in international wages based on this data. Section 1.7 describes the methodology employed and the empirical results. Section 1.8 summarises the results and concludes.

1.2 Growth and Wage Convergence in the Neoclassical Solow-Swan Model

Conditional convergence is a key implication of the Solow-Swan model, whose role as a reference point and cornerstone in modern growth theory has been firmly established. We briefly review the Solow-Swan model and the convergence hypothesis as a theoretical background to the subsequent empirical analysis. As our interest is in understanding real *wage* convergence as an indicator of a population's well-being, thus going beyond the highly aggregated measures of income such as GDP per capita, we derive an expression for wage convergence in the Solow-Swan model for the case of a Cobb-Douglas production function.

1.2.1 Theoretical Background: Growth and Wages in the Standard Neoclassical Solow-Swan Model

The Solow-Swan model assumes a neoclassical production function with constant returns to scale and positive but diminishing returns to factor inputs. The constant rates of savings s, population growth n and technological progress g are taken as exogenous, while the factor inputs capital and labour are paid their marginal products. We assume a Cobb-Douglas production function

$$Y = K^{\alpha}(AL)^{1-\alpha} \tag{1.1}$$

with aggregate income Y, physical capital K, $A > 0$ the level of labour augmenting technology, L labour and capital's constant share of income α, with $0 < \alpha < 1$. AL denotes the effective units of labour and grows at the rate $n + g$. As we are interested in per worker effects, we use the equivalent intensive form

$$y = f(k) = k^{\alpha} \tag{1.2}$$

with capital per effective unit of labour $k \equiv K/AL$, and output per effective unit of labour $y \equiv Y/AL$. Income is shared by workers and capital, which in a competitive economy are paid their marginal products in the form of rents[1] r and wages w:

$$\begin{aligned} r &= \partial Y/\partial K = f'(k) = \alpha k^{\alpha-1} > 0 && (1.3) \\ w &= \partial Y/\partial AL = f(k) - k \cdot f'(k) = (1-\alpha)k^{\alpha} > 0 && (1.4) \end{aligned}$$

The evolution of the capital stock per effective worker over time $\dot{k}$ is the fundamental differential equation of the Solow-Swan model. While savings increase the capital stock per effective worker, capital depreciation at the rate δ, population growth at the rate n and technological progress at the rate g diminish k in each period:

$$\begin{aligned} \dot{k} &= s \cdot f(k) - (n + g + \delta) \cdot k && (1.5) \\ &= s \cdot k^{\alpha} - (n + g + \delta) \cdot k && (1.6) \\ \dot{k}/k &= s \cdot k^{(\alpha-1)} - (n + g + \delta) && (1.7) \end{aligned}$$

A steady state is reached, when k and y grow at constant rates. Due to the diminishing returns assumed in the neoclassical production function, this can only hold if $\dot{k} = 0$, i.e. when the growth rates of k and y are zero, while K and Y grow at the rate of $n + g$. From equation (1.5) it follows that the steady state level of k is determined by

$$s \cdot f(k^*) = (n + g + \delta) \cdot k^* \tag{1.8}$$

[1]Note that depreciation δ on capital lowers the net rate of return on a unit of capital to $r - \delta$.

For the Cobb-Douglas production function capital per effective worker in the steady state therefore equals

$$k^* = \left(\frac{s}{n+g+\delta}\right)^{1/(1-\alpha)} \tag{1.9}$$

While $k < k^*$ the effect of savings in increasing the amount of capital per effective worker is larger than the effect of population growth, technological progress and depreciation in reducing capital per effective worker, $(n+g+\delta)\cdot k < s \cdot f(k)$. Thus, the growth rate of k is positive: $\dot{k}/k > 0$. However, as k rises the diminishing returns of the production function mean that capital's average product is declining, thus slowing the effect of savings such that the growth rate of capital per effective labour $\dot{k}/k$ declines along the path towards the steady state:

$$\begin{aligned} \partial(\dot{k}/k)/\partial k &= s \cdot [f'(k) - f(k)/k]/k && (1.10)\\ &= (\alpha-1)\cdot s \cdot k^{a-2} < 0 && (1.11) \end{aligned}$$

As income y is directly related to k, it is straightforward to derive conditional convergence for y as k rises along the transition path:

$$\begin{aligned} \dot{y}/y &= [f'(k)\cdot \dot{k}]/f(k) && (1.12)\\ &= \alpha \cdot (\dot{k}/k) = \alpha \cdot [s \cdot k^{\alpha-1} - (n+g+\delta)] && (1.13)\\ \partial(\dot{y}/y)/\partial k &= \alpha(\alpha-1)\cdot s \cdot k^{a-2} < 0 && (1.14) \end{aligned}$$

Thus, the growth rate of output $\dot{y}/y$ mimics $\dot{k}/k$, and declines as k rises along the path towards the steady state for $0 < k < k^*$: For two structurally similar countries which have the same levels for the parameters n, s, g and δ, and that produce with the same production function (i.e. they share the same steady state), growth rates of income will be higher for the country with the lower level of capital per unit of effective labour k, i.e. for the poorer country. The resulting catch-up process shown above for the Cobb-Douglas production function more generally holds for a broad range of neoclassical production functions, see Barro and Sala-i-Martin (2004, p. 39ff). The implication of "conditional convergence" (referred to as "conditional" because it requires the assumption that the countries share the same steady state, so convergence is conditional on the structural similarity of these countries) forms the basis for the large empirical literature analysing cross-country growth rates, which is reviewed in section 1.4.

What are the implications for wages? The pace of wage growth $\partial(\dot{w}/w)/\partial k$ depends on the sign of the third derivative $f'''(\cdot)$ of the production function. As the third derivative's sign cannot be generally determined for neoclassical production functions, we again use the explicit Cobb-Douglas form. As effective labour receives a fixed share of income $(1-\alpha)$ for the Cobb-Douglas production function, wages as defined in equation (1.4) should grow in line with income along the transition towards the steady state. Using equations (1.4) and (1.6)

$$\begin{aligned} \dot{w}/w &= [-k \cdot \dot{k} \cdot f''(k)]/[f(k) - k \cdot f'(k)] && (1.15) \\ &= \alpha \cdot [s \cdot k^{\alpha-1} - (n+g+\delta)] && (1.16) \\ \partial(\dot{w}/w)/\partial k &= \alpha(\alpha-1) \cdot s \cdot k^{a-2} < 0 && (1.17) \end{aligned}$$

Thus the pace of wage growth declines in step with the decline in the pace of income growth as the economy approaches the steady state.[2,3]

The importance of labour augmenting technological progress in the Solow-Swan model is evident. Without it income per capita stagnates once a country has reached its steady state. It is important to note that while technological innovations are often perceived as being the key driver of productivity, in the Solow-Swan model context other factors can equally be interpreted as impacting the technology parameter A: Weak government institutions that fail to protect citizens, fragile property rights and a failure to establish the rule of law can be detrimental to productivity growth. Corruption, a lack of stability, excessive taxes and other market distortions can result in an inefficient allocation of resources with an economic effect equivalent to that of slower technological progress resulting in a lower steady state level of per capital income in the Solow-Swan model.

Finally, we recall the speed of convergence σ_k, which measures by how much proportionally the growth rate is reduced as capital per effective worker rises:[4]

$$\sigma_k \equiv -\frac{\partial(\dot{k}/k)}{\partial log(k)} \qquad (1.20)$$

From equation (1.7) we rewrite the growth rate of capital $\dot{k}/k$ as a function of $\log(k)$ and

[2] The slowing of wage growth as k rises can also be shown by looking at the derivatives of wages with respect to k:

$$\begin{aligned} \partial w/\partial k &= \alpha(1-\alpha) \cdot k^{a-1} > 0 && (1.18) \\ \partial^2 w/\partial k^2 &= -\alpha(1-\alpha)^2 \cdot k^{a-2} < 0 && (1.19) \end{aligned}$$

[3] Note that the wages in equation (1.4) refer to the wages of the effective units of labour AL.

[4] This definition of the speed of convergence σ_k, which uses $logk$ in the denominator, is common in the literature, see, for instance, Barro and Sala-i-Martin (2004, p. 56ff). In contrast to the formulation with just k in the denominator, this definition emphasises on the proportionality of the change in growth rate.

subsequently determine the speed of convergence by taking the derivative with respect to $\log(k)$:

$$\dot{k}/k = s \cdot e^{-(1-\alpha)\cdot \log(k)} - (n+g+\delta) \quad (1.21)$$

$$(1.22)$$

$$\sigma_k \equiv -\frac{\partial(\dot{k}/k)}{\partial log(k)} = (1-\alpha)\cdot s \cdot k^{-(1-\alpha)} \quad (1.23)$$

For k close to k^* around the steady state $s \cdot k^{-(1-\alpha)} \approx (n+g+\delta)$, so that the speed of convergence close to the steady state can be approximated by

$$\sigma_k \approx (1-\alpha)\cdot(n+g+\delta) \quad (1.24)$$

The speeds of convergence $\sigma_y = \frac{\partial(\dot{y}/y)}{\partial log(y)}$ for income y and $\sigma_w = \frac{\partial(\dot{w}/w)}{\partial log(w)}$ for wages w are calculated by expressing equations (1.13) and (1.16) with respect to $log(y)$ and $log(w)$, using $k = y^{1/\alpha}$ and $k = (\frac{w}{1-\alpha})^{1/\alpha}$:

$$\dot{y}/y = \alpha \cdot (\dot{k}/k) = \alpha \cdot s \cdot e^{-(1-\alpha)(1/\alpha)\cdot \log(y)} - (n+g+\delta) \quad (1.25)$$

$$\dot{w}/w = \alpha \cdot (\dot{k}/k) = \alpha \cdot s \cdot e^{-(1-\alpha)(1/\alpha)\cdot[\log(w) - \log(1-\alpha)]} - (n+g+\delta) \quad (1.26)$$

Taking the derivates with respect to $log(y)$ and $log(w)$, respectively, gives $\sigma_y = \sigma_w = \sigma_k$.

1.2.2 Theoretical Background: Growth and Wages in the Augmented Neoclassical Solow-Swan Model

In their seminal 1992 paper Mankiw, Romer and Weil tested "whether the Solow growth model is consistent with the international variation in the standard of living". Mankiw et al.'s (1992) innovation was twofold: First, they focused attention on the fact that the Solow-Swan model does not imply absolute convergence, i.e. poorer countries growing faster than rich ones, but rather that it implies convergence conditional on the countries having the same steady states. Second, they introduced an augmented Solow model with human capital as a further productive factor. The augmented model is structurally similar to the Solow-Swan model presented in the last section. Replacing the production function in equation (1.1), Mankiw et al. (1992) set

$$Y = K^{\alpha} H^{\beta} (AL)^{1-\alpha-\beta} \quad (1.27)$$

with H the stock of human capital, and $\alpha + \beta < 1$ implying diminishing returns to physical and human capital. With $h = H/AL$, s_k the share of income invested in physical capital and s_h the share of income invested in human capital, the equation of motion (1.5) is replaced by the new differential equations describing per effective unit of labour accumulation of physical and human capital:

$$\dot{k} = s_k \cdot f(k) - (n + g + \delta) \cdot k \tag{1.28}$$

$$\dot{h} = s_h \cdot f(k) - (n + g + \delta) \cdot h \tag{1.29}$$

whereby physical and human capital depreciate at the same rate δ. Therefore, in the steady state

$$k^* = \left(\frac{{s_k}^{1-\beta} \cdot {s_h}^{\beta}}{n + g + \delta} \right)^{1/(1-\alpha-\beta)} \tag{1.30}$$

$$h^* = \left(\frac{{s_k}^{\alpha} \cdot {s_h}^{1-\alpha}}{n + g + \delta} \right)^{1/(1-\alpha-\beta)} \tag{1.31}$$

To generate an equation that can be empirically tested, Mankiw et al. (1992) substitute the steady state conditions (1.30) and (1.31) into the production function (1.27) to get an expression that shows the relationship in the neighbourhood of the steady state between income per capita, a term comprising technology $\ln A$, the growth rate of technological progress g, the logarithm of population growth, technological progress and depreciation, and the logs of the savings ratios for physical and human capital:[5]

$$\begin{aligned} \ln \left[\frac{Y}{L} \right] = \ln A - \frac{\alpha + \beta}{1 - \alpha - \beta} \cdot \ln (n + g + \delta) \\ + \frac{\alpha}{1 - \alpha - \beta} \cdot \ln s_k + \frac{\beta}{1 - \alpha - \beta} \cdot \ln s_h \end{aligned} \tag{1.32}$$

For each country $A = A(0) \cdot e^{gt}$, where $A(0)$ is interpreted as representing not only the starting level of technology, but also environmental factors such as resource endowments, the quality of public institutions, property rights and the rule of law, climate, and so on. All countries share the same technology growth rate g, which is assumed to reflect the overall "advancement of knowledge" (Mankiw et al., 1992, p. 410). $A(0)$ is unobserved and thus decomposed into a constant across all countries a and a country-specific shock ϵ which enters the error term. Mankiw et al. (1992) assume that the regressors are independent of ϵ, i.e.

[5] Mankiw et al. (1992) also propose an alternative formulation in which the level of human capital h^* rather than its growth rate is included as a regressor.

exogenous. Clearly, if the regressors are in fact endogenous, then OLS estimates are likely to be inconsistent. This is a key point of criticism of Mankiw et al.'s (1992) formulation, as $A(0)$ in ϵ is likely to be correlated to the rates of savings in physical and human capital, thereby biasing the OLS coefficient estimates and R^2 upwards. However, Mankiw et al. (1992) state that finding suitable instrumental variables would be "a formidable task", and thus use OLS for estimation.[6]

This augmented model in which the regression coefficients are functions of factor shares forms the basis for much of the empirical estimation of cross-country growth regressions over the past 20 years. With their estimation Mankiw et al. (1992) are able to explain 3/4 of variation in cross-country data and their estimated coefficients are around the levels suggested by the factor shares, which they estimate to be around 1/3 for physical capital's share of income α and 1/3 to 1/2 for human capital's share of income β. In spite of subsequent challenges to the Mankiw et al. (1992) methodology – and in particular the critique of omitted variable bias and reverse causality – this analysis firmly cemented the role of population growth, physical and human capital and technological progress as key elements in economic growth theory and empirics.

We formulate an equivalent relationship for wages. In contrast to the approach in section 1.2.1, we differentiate with respect only to L, not to the effective units of labour AL, thereby following the approach selected by Mankiw et al. (1992). Labour is paid its marginal product, such that

$$w = \frac{\partial Y}{\partial L} = (1 - \alpha - \beta) \cdot A \cdot (k^{\alpha} \cdot h^{\beta}) \tag{1.33}$$

Substituting conditions (1.30) and (1.31) for k and h and takings logs gives

$$\begin{aligned} \ln(w) = \ln(1 - \alpha - \beta) + \ln A - \frac{\alpha + \beta}{1 - \alpha - \beta} \cdot \ln(n + g + \delta) \\ + \frac{\alpha}{1 - \alpha - \beta} \cdot \ln s_k + \frac{\beta}{1 - \alpha - \beta} \cdot \ln s_h \end{aligned} \tag{1.34}$$

[6] In their extensive review of a wide range of hurdles in econometrics encountered in empirical growth studies, Durlauf et al. (2005, p. 637ff) discuss instrumental variable use in detail, and report: "In our view, the belief that it is easy to identify valid instrumental variables in the growth context is deeply mistaken. We regard many applications of instrumental variable procedures in the empirical growth literature to be undermined by the failure to address properly the question of whether these instruments are valid, i.e. whether they may plausibly be argued to be uncorrelated with the error term in a growth regression. When an instrument is invalid, instrumental variables estimates will of course be inconsistent. Not enough is currently known about the consequences of "small" departures from validity, but it is certainly possible to envisage circumstances under which ordinary least squares would be preferable to instrumental variables on, say, a mean square error criterion." Their critical assessment of the alternative approach thus provides some pragmatic support to Mankiw et al.'s (1992) use of OLS.

The equation for wages differs from equation (1.32) for income only in the intercept, which is expected to be lower due to $\ln(1-\alpha-\beta) < 0$. Thus, in the steady state we would expect the wage rate to be in a parallel situation to income per capita as regards its relationship to population growth, physical and human capital accumulation and technological progress. To generate a testable expression for the wage rate w outside of the steady state, we follow an analog approach to that used by Mankiw et al. (1992) for income per effective worker.

Note that along the transition path the log wage in period t can be portrayed as a linear combination of the logs of the initial wage $w(0)$ and the steady state wage w^* with σ_w the speed of convergence:

$$\ln(w(t)) = (1-e^{-\sigma_w t})\ln(w^*) + e^{-\sigma_w t}\ln w(0) \tag{1.35}$$

Substituting for $\ln(w^*)$ from equation (1.34), subtracting $\ln w(0)$ from both sides and dividing by t gives the expression for the growth rate of wages:

$$\begin{aligned}\frac{1}{t}\cdot\ln\left(\frac{w(t)}{w(0)}\right) = \frac{(1-e^{-\sigma_w t})}{t}\cdot\Bigg[&\ln(1-\alpha-\beta)+\ln A-\ln w(0)\\ &-\frac{\alpha+\beta}{1-\alpha-\beta}\cdot\ln(n+g+\delta)\\ &+\frac{\alpha}{1-\alpha-\beta}\cdot\ln s_k+\frac{\beta}{1-\alpha-\beta}\cdot\ln s_h\Bigg]\end{aligned} \tag{1.36}$$

Thus, wage growth on the transition path towards the steady state is a function of the determinants of the steady state n, g, δ, s_k and s_h, the initial wage rate $w(0)$ and A, which incorporates the starting level and exogenous rate of technological progress, but also environmental factors which impact productivity, as described earlier.

In the neighbourhood of the steady state the speed of convergence σ_k can now be calculated for the augmented model, analog to the calculation for equation (1.24) as:

$$\sigma_k \approx (1-\alpha-\beta)\cdot(n+g+\delta) \tag{1.37}$$

This implies a slightly slower speed of convergence than in the Solow-Swan model without human capital. In fact, with the inclusion of human capital and using the customary parameter values $\alpha=\beta=1/3$ and $n+g+\delta=0.06$, the speed of convergence close to the steady state in this model would be $\sigma_k=0.02$, which is close to the convergence rates for income that have been observed empirically.[7] This would imply a slow convergence process

[7] Cf. Mankiw et al. (1992, p. 423) and Barro and Sala-i-Martin (2004, p. 60).

in which a country could halve the distance to its steady state only in about 35 years.

1.3 Other Frameworks for International Wages

Theories of economic growth are not the only frameworks that suggest how international wages might evolve. Of the two major alternative frameworks, one is based on migration theory, while the second is based on trade theory.

1.3.1 Wages in the Context of Migration

The idea that migration and differences in real wages are fundamentally related lies at the core of Sjaastad's (1962) migration theory in which migration takes place if a person's present value of returns to migration exceed the present value of its costs. As real wages determine economic utility to a large extent, migrants are expected to leave low-wage regions and head towards high-wage regions, in line with Borts and Stein's (1964) depiction of rural-to-urban migration. The long-run equilibrium implies wage convergence: The falling supply of labour would raise wages in low-wage areas over time, while the additional workforce would result in downward pressure on wages in high-wage regions.

Later wage theories incorporate broader concepts of utility, to include weather, rents, beauty and pleasantness of the environment, convenience of the location and other amenities, see, for instance, Graves and Linneman (1979), Rosen (1979) and Roback (1982, 1988). Roback describes a spatial equilibrium in which households' utility and corporate profitability are equalised over different regions, i.e. a particularly poor natural environment (e.g. poor weather, isolated location, pollution) would be offset by lower rents, and a willingness of companies to pay higher wages to the extent that they benefit financially from this domicile. In this context the implication of wage convergence disappears – on the contrary, differential wages can be a component of utility compensation between regions in equilibrium. However, the potential equilibria are manifold and the expectations with regard to wage convergence or divergence are ambiguous:[8] If, for example, rents adapt in the long run to perfectly absorb the utility (or disutility) of household and company amenities, then wages can again be expected to converge in this framework.

Clearly, any impact of migration on international wages depends on the extent of migration. Strong support for migration as a driver for international wage convergence comes from economic historians such as Jeffrey G. Williamson. For example, the period 1870 to 1914 is referred to as an "age of mass migration", during which, "in the absence of

[8] See Graves (2013).

quotas, (...) the numbers who elected to move were enormous" (Taylor and Williamson, 1997). Notwithstanding the difficulty in estimating a counterfactual of wage development for the case if no migration had taken place, Taylor and Williamson (1997) provide estimates from a partial equilibrium analysis that point to migration accounting "for very large shares of the convergence in GDP per worker and real wages". Williamson (1996) summarises evidence for the significant role of mass migration for convergence, among others, for Ireland, Sweden, the UK and the US in the pre-1914 period. While Williamson (1996) generally points to the de-globalisation and autarky ambitions in the war and interwar periods as a key reason for the cessation of convergence during that period, Taylor and Williamson (1997) specifically point to the introduction of migration quotas and other barriers as potential causes.

The stark limitations on legal migration that have been put in place during the past decades stand in contrast to the increased illegal migration in some parts that have been aided by transportation's lower cost and increased availability. Nonetheless, a reduced impact today for migration on international wage trends relative to the pre-1914 era seems plausible. Trade volumes, on the other hand, have tended to increase in most parts of the world – the next section discusses wages in the context of international trade and limited labour mobility.

1.3.2 Wages in the Context of Trade

With hurdles to international labour mobility that can be substantial, the wage arbitrage effects through migration described in the above section cannot be expected to fully unfold in the current international context. The Heckscher-Ohlin model of trade finds that regional differences in factor endowments combined with precisely these limitations to the mobility of production factors can, in fact, be a driver for trade. Further, under certain conditions trade in goods can act as a substitute for factor mobility, implying that factor prices will converge even without factor mobility between countries. This occurs because as countries engage in trade they increase exports of goods that intensively use the factors with which they are highly endowed, and import more goods intensive in factors with which they are only poorly endowed. As algebraically described by Stolper and Samuelson (1941), in each country this increases the relative demand for the more abundant (and cheaper) factor, while demand for the scarcer (and more expensive) factor falls, putting pressure on its price. In this way trade in goods can induce via a shift in factor demand an adjustment of factor prices. Further, under specific conditions an invertible mapping exists between the vector of goods prices and the vector of factor prices, through which goods prices uniquely set factor prices across regions. This result, termed the Factor Price Equalisation

(FPE) theorem (cf. Samuelson, 1948, and Lerner, 1952), has become a key component in international trade theory and reflects intuitive expectations of how trade could affect wages.

Nonetheless, tremendous divergences in international wages persist. From a theoretical perspective the restrictive conditions of the original FPE theorem[9] within the context of the 2x2x2 Heckscher-Ohlin model are an important first constraint that might explain these divergences. However, extensions of Heckscher-Ohlin to more generalised contexts including more goods, factors, non-traded goods and market imperfections[10] and in particular specifications of general equilibrium models *where FPE does not necessarily hold* suggest that FPE *will nearly hold* between trading partners.[11] This Near FPE applies across a wide range of production functions (see Thompson, 1990, and Thompson, 1997).

Thus, FPE might rather be viewed as a longer term tendency (see Hicks, 1959, p. 267), much like Purchasing Power Parity (PPP), which does not hold in the short term but is considered a valuable guide for longer term trends. Reflecting this, Samuelson (1971) rightly directed the discussion towards Factor Price Convergence (FPC): The critical question is whether factor prices converge over time as trade barriers and transportation costs fall – if so, the mechanism at the core of the FPE theorem must be at work.[12]

International wage convergence therefore follows from the Heckscher-Ohlin framework of trade and the related FPE and FPC theories.

1.4 Overview of Empirical Studies: Growth, Wages and Convergence

1.4.1 Overview of Empirical Studies: Growth and Convergence

In the appendix of their extensive review on growth econometrics, Durlauf et al. (2005) list 145 explanatory variables used in growth regressions since the early 1990s, and cite over 80 empirical studies of cross-country growth analysis. In this section we summarise this vast empirical growth literature, present a concise review of the most common empirical approaches and put these in the context of the theoretical models discussed in the previous sections. While this section reviews the empirical literature on *growth* and convergence, the next section reviews the empirical literature on *wages* and convergence.

[9] Cf. section 2.2 for a more detailed discussion.
[10] See among others Ethier (1974), Chang (1979), Takayama (1982) and Thompson (1987).
[11] Rassekh and Thompson (1993, p. 6).
[12] The Specific Factors model with homothetic demand (see, for instance, Samuelson, 1971) in which FPC occurs with free trade is frequently considered the shorter term version of FPE in the Heckscher-Ohlin context. See also Thompson (1994) for an alternate formulation of the Specific Factors model; here Near FPE is a robust result.

Empirical studies into growth and convergence can be classed into two broad categories: The first group is based on the fundamental growth determinants that make up the Solow-Swan model: physical and human capital, population growth, depreciation, and technological progress. These studies aim to confirm or disprove the theoretical models, to provide numeric estimates for model parameters, and to improve the quality of the early empirical results by 1) better matching the Solow-variables with empirical equivalents, e.g. improved choice of measures to reflect human capital or technological progress, 2) using superior data methods, e.g. controlling for outliers, 3) improving regression fit, e.g. using variables in levels versus differences, and 4) using more sophisticated econometric techniques, e.g. instrumental variables and semiparametric methods.

An important example in this context is the modelling of human capital in cross-country growth regressions. In response to Mankiw et al.'s (1992) augmented Solow-Swan model, Benhabib and Spiegel (1994) refute the role of human capital *accumulation*, finding that log differences in human capital over time have no statistically significant effect on growth in their regressions spanning 1965 to 1985. However, they find that the *stock* of human capital is positively related with growth. But Temple (1999) finds that Benhabib and Spiegel's (1994) result for human capital accumulation is due to outliers in the data, and when these are removed the coefficient on human capital accumulation becomes positive and significant. Krueger and Lindahl (2001), on the other hand, find the *stock* of human capital not to be statistically significant for growth when the sample is restricted to OECD countries. Thus, the *stock* of human capital might only be relevant during the catch-up process, not for comparisons of countries at similar levels of per capita income. Aghion and Howitt (1998) make a similar distinction of two frameworks of modelling of human capital in the endogenous growth literature, which equally influenced the empirical approaches: The Lucas approach, based on Lucas (1988), argued that differences in growth rates across countries are primarily due to differences in the rates of human capital accumulation, much in line with neoclassical growth theory. In contrast, the Nelson-Phelps approach (1966) sees the primary role of human capital not in an increase in labour's productivity, but rather in the ability of workers to adapt: to disruptions, to structural changes and to the introduction of new technologies. Thus, in their model human capital does not feature at all in the production function, but rather in the function governing the evolution of technology. Thus, the question of whether human capital should be included in levels or first differences, or whether it should be included in the standard production function model at all has been a key element of discussion in the literature on cross-country growth determinants.[13]

[13]See Engelbrecht (2001) for a discussion on the Lucas versus Nelson-Phelps comparison.

The second group reflects the view that while the Solow-Swan variables are clearly important to growth, they are only part of the explanation. This group thus extends the Solow-Swan framework by adding additional control variables. The resulting regressions are sometimes called "Barro regressions", due to Robert J. Barro's extensive application of this structure of regression for identifying drivers of economic growth. These additional variables can be seen as either additional measures to ensure that one is controlling sufficiently for structural equivalence, i.e. the same steady state. Or, alternatively, the additional variables can be founded on alternative theoretical models, in particular models of endogenous growth that started with Romer (1987, 1990), Lucas (1988) and Rebelo (1991). Most empirical studies nonetheless include the Solow-Swan variables as a baseline.

Disappointingly for a field that has received so much attention over the past quarter of a century, only a limited consensus has emerged on which of the 150+ variables that have been investigated can be considered robust and statistically significant determinants of economic growth. Among the better-established variables one might venture to mention are those identified by Barro and Sala-i-Martin (2004, Chapter 12): Initial per capita GDP, male upper-level schooling, life expectancy at age one (reciprocal), total fertility rate, government consumption ratio, rule of law, democracy, democracy squared, openness ratio, changes in the terms of trade, investment ratio, inflation rate, and period dummies. A fundamental difficulty in this respect is that economic growth analysis suffers from "theory open-endedness" (Brock and Durlauf, 2001), i.e. that any number of growth theories can be mutually compatible, existing side by side without creating logical inconsistencies, thus resulting in a high number of potential explanatory variables.

Probably the largest hurdle to the identification of empirically salient drivers among the proposed regressors, and to growth econometrics more generally, is the limited number of countries in the world. With more explanatory variables having been proposed than countries exist for which suitable data is available, the robust choice of control variables is statistically challenging. Durlauf et al. (2005) recall that researchers thus "typically emphasize a single model (or a small set of models) and then carry out inference as if that model had generated the data. Standard inference procedures based on a single model and which are conditional on the truth of that model can grossly overstate the precision of inferences about a given phenomenon. Such procedures ignore the uncertainty that surrounds the validity of the model." Many different techniques have been employed in the attempt to improve identification, including Bayesian, pseudo-Bayesian and frequentist model averaging estimators, general-to-specific modelling, principal components augmented regressions and adaptive lasso sequences, but "genuine progress" seems hard to achieve (Durlauf et al., 2005).

The small sample size in cross country growth regressions is also a hurdle when addressing measurement error and outliers, and when allowing for parameter heterogeneity, e.g. with interaction terms, nonlinearities or semiparametric methods. Additionally, regressor endogeneity seems plausible in many cross-country growth regressions. In the context of theory open-endedness Brock and Durlauf (2001) point out that for an instrument to be classified as valid, a positive argument is needed that the proposed instrument cannot be a growth determinant (e.g. in a mutually compatible theory), and that it cannot be correlated with an omitted regressor. Given these high hurdles, Durlauf et al. (2005) challenge the validity of instruments proposed in previous empirical studies.[14] Finally, the observation that empirical results are rather sensitive to changes in the start and end years is not comforting. Business cycles can easily distort the measurement of long term economic growth, depending on the start and end points of measurement, resulting in potential inconsistencies for results spanning different time periods.

While the identification of robust and statistically significant growth determinants is still unfinished work, initial income per capita – the regressor capturing conditional convergence – has firmly secured its place in cross country growth regressions. In fact, there has been remarkable consistency in finding statistically significant coefficients with negative signs indicating convergence after controlling for differences in the steady states. Empirical convergence rates have mostly been found to be between one and three percent, clustering around two percent (cf. Barro and Sala-i-Martin, 1992; Durlauf et al., 2005). These results are in the order of magnitude that is compatible with Mankiw et al.'s (1992) augmented Solow-Swan model in which the convergence rates are related to the exogenous model parameters. Conditional convergence – as basic property of the Solow-Swan model – is thus widely accepted to have considerable explanatory power for economic growth across countries and regions.

1.4.2 Overview of Empirical Studies: Wages and Convergence

Most empirical studies on wage differentials focus on differentials within a country: between rural and urban regions, between the genders or races within a country, between the skilled and the unskilled. For studies involving more than one country the comparability and availability of data becomes a significant bottleneck. Thus, studies comparing wages of different countries mostly focus on few specific countries in a specific context, such as wage convergence of EU candidate countries relative to member states (see, for instance, Egger, 2006), or wage convergence between Mexico and the US after NAFTA (see, for instance,

[14] See Durlauf et al. (2005) for an in-depth discussion on the challenges in growth econometrics.

Hanson, 2003). The lack of datasets of comparable international wage data for a wide range of countries have made tests of international wage convergence scarce. The following three studies are rare examples of empirical interest in international wage convergence.

The economic historian Jeffrey G. Williamson (1995) introduces a dataset of purchasing-power-parity-adjusted real wage rates for unskilled labour from the mid-nineteenth century for 15 countries that participated in the early international commodity markets. In a number of studies based on this dataset and extended versions of it, he and co-authors confirm wage convergence (and factor price convergence more broadly) for the periods during which strong globalization trends dominated: 1870-1913, which is labelled a "regime of dramatic convergence" (Williamson, 1995), and 1946-1988. During the World Wars and interwar years 1914-1945 real wage convergence ceased, with 1934-1945 even being characterised by divergence as real wages in the United States surged relative to the other regions.[15] Williamson (1996) puts forward that two major forces of globalisation, migration and commodity price convergence, are the key drivers of wage convergence during these periods. The small sample size limits the significance of regression analysis and is not the focus of the study. Nonetheless, it is informative that in an unconditional convergence equation Williamson (1996) reports a "rate of real wage convergence between 1870 and 1890 of 1.2 percent per annum, and about 1 percent per annum over the 1870 to 1913 period as a whole."

In a study by OECD economists focusing on the factor content of trade, Stone et al. (2011) report finding "no conclusive statistical evidence of convergence in real wages" using data for the period 1984 to 2002. They use Freeman and Oostendorp's (2000) updated Occupational Wages around the World (OWW) database and regress the percentage change in wages on the logarithm of initial GDP per capita (not initial real wage), as well as adding a measure for the level of a country's initial openness, random effects by country and fixed effects by occupation and separately by sector. Their regression results show that estimated coefficients were not systematically significant, and the signs on the coefficients changed frequently for the different specifications.

Morris (2009) finds evidence for convergence of compensation costs measured in US dollars (market exchange rate) for a selected group of countries, after these have been segmented into "convergence clubs", but not prior to this segmentation and not for all clubs. The analysis is based on the log t test described in Phillips and Sul (2007) using hourly manufacturing compensation costs from the US Bureau of Labor Statistics (BLS) over the period 1975-2006. Compensation data prior to the mid-1990s is missing for five of

[15] See, among others, Williamson (1995), Williamson (1996), O'Rourke et al. (1996) and Taylor and Williamson (1997).

the included emerging markets. Evidence for PPP-adjusted compensation costs provided somewhat weaker support to the convergence hypothesis than when using market exchange rates.

Empirical evidence on the question of wage convergence thus remains sparse, and the conclusions unclear in large part due to limited data availability. We use the wage data from the UBS Prices and Earnings surveys to contribute to filling this gap. These data, described in section 1.5 below and in more detail in appendix A, have significant advantages as compared to data used in the above-mentioned studies as the former are based on an identical survey of profession-level wage data around the world.

1.5 Data Description: UBS Prices and Earnings Survey

The UBS Prices and Earnings surveys of international prices and wages have been conducted every three years since 1970, producing a dataset, which displays high consistency over time and good global comparability, as in each country the survey was conducted contemporaneously with an identical questionnaire and comparable methods.[16] The survey collected earnings data from professions in the manufacturing and services sector from 31 cities around the world in 1970, growing to 71 cities in 2009. Of the 35 cities that appeared in every, or nearly every survey, 17 are in Europe, six in Latin America, five in North America, four in Asia, one in Africa, one in the Middle East and one in Australia.[17]

Professions surveyed were selected based on two criteria. First, they had to constitute a representative cross-section of the workforce in the manufacturing and service sectors.[18] Second, the professions needed to be common in most metropolitan centres around the world and it had to be possible to define and consistently capture the data globally. We use the survey measure "gross annual income"[19] which we adjust for purchasing power using the consumption-based PPP conversion rates from the Heston et al. (2012) Penn World Tables, version 7.1.

Except for the expansion in the number of cities and professions surveyed, the questionnaire on earnings has remained largely unchanged since 1970. In each survey year UBS recruited local surveyors – mostly three to four independent surveyors per city could be

[16]See table A.1 in the appendix for information on the survey periods.
[17]See table A.3 in the appendix for information on which cities were included in the surveys.
[18]For most years data on twelve professions are available.
[19]This measure is defined in the survey as "Gross annual income (sum of hourly, weekly or monthly earnings) taking into account family status and tax allowances including all fringe benefits such as profit participation, bonuses, vacation money, additional monthly salaries as bonus payments, allowances for children etc. but excluding overtime compensation."

found. The surveyors received detailed instructions on how to proceed with the survey, including the time frame during which the survey was to be conducted. With regard to the labour market information, surveyors were instructed to request data from representative companies of the specific sectors. The survey data is therefore not micro-level data, but should rather be interpreted as representative agent data for each profession in each city. The data for each survey item was then averaged across surveyors. Three examples of descriptions of the professions as in the survey are shown below:

- Skilled industrial worker: Skilled mechanic ("worker") with vocational training and about 10 years experience with a large company in the metal working industry; about 35 years old, married, two children.

- Female sales assistant: Female sales assistant ("Saleswoman") employed in a women's clothing section ("ladies wear") of a large department store; sales training plus some years of sales experience; about 20 to 25 years old, single.

- Bus driver: Employed by the municipal transportation system (also "public", or "municipal transport operator"), about 10 years driving experience; about 35 years old, married, two children.

As the UBS Prices and Earnings survey does not provide country estimates, but only provides data for selected large cities, rural occupations, in particular in agriculture, are not included and any differential development between wages in rural and urban environments are not reflected in the data.[20]

Each profession is assigned a skill level by the author (not the survey) based on the following breakdown:
Skill level 1: Obligatory / statutory schooling only; largely unskilled labour; very limited training.
Skill level 2: Obligatory / statutory schooling plus full apprenticeship or extensive practical training, or completed high school and some practical training.
Skill level 3: Completed high school and university or college education.

Skill level 1 professions therefore include bus drivers, factory and textile workers, saleswomen, and construction workers. Skill level 2 professions include secretaries, auto mechanics, bank tellers or credit clerks, cooks and skilled industrial workers. Finally, skill

[20] Many databases used for country comparisons must also make do with data primarily from urban areas – less affluent regions and rural areas are frequently poorly represented (see, for instance, O'Connor, 2008, p. 3, and Van Ark and Monnikhof, 2000, p. 6ff).

level 3 professions include primary school teachers, electrical engineers and department managers in industry.

These data have a number of useful characteristics, that are lacking in the wage data used in the studies described in section 1.4.2. In contrast to data which stems from many different country-specific sources (such as data from industry surveys, trade unions, censuses, government institutions etc.) as is the case for the OWW wage data and the BLS compensation cost data in which the wage measure can vary substantially, the UBS Prices and Earnings data is comparable not only across countries, but also over time. The consistent definition of professional wages in the UBS survey and the attribution of skill levels make significantly more differentiated analyses possible. This is an advantage as compared to databases that have only data for specific groups, e.g. "manufacturing" workers for the BLS data and "unskilled" workers in the case of Jeffrey G. Williamson's (1995) data. The BLS data reflect compensation costs and not wages directly, making them less suitable for standard-of-living comparisons. Clearly, the Williamson (1995) data, which spans both the 19th and 20 centuries, is based to a large extent on historical documents, implying that consistency over time and comparability across countries is likely somewhat poorer. The lack of consistent sources over time and space is a significant weakness of the OWW database.[21]

The key strength of the UBS Prices and Earnings wage data is thus the consistent definition of workers and professions across all cities and over time, enabling the attribution of broadly comparable skill levels across all data. The econometric analysis can thus use not only the common dimensions of time, location (region), and wage level, but also those of profession and skill level. These dimensions are important given the changes in skill premia observed in several countries over the past years, which would suggest that analysing only particular skill groups might not provide a very complete picture.

In terms of limitations, the data do not reflect differences in the structure of labour markets, as household survey data would. This wage data therefore cannot be assumed to represent all professions within a city, but only certain professions, which exist in all cities. For our current application this limitation does not seem serious as the professions for which data is provided represent a good cross section in both manufacturing and services, and differing skill levels.

[21] Note that the technical document to the OWW database (Oostendorp, 2005) reports that a significant share of the values are imputed due to low response rates - on average countries reported wages only for 8.5 out of 21 possible years, and did not necessarily report data for all occupations even when they did report. Also, countries report wages differently, e.g. based on different sources such as employer surveys, household surveys, collective bargaining contracts and legislated pay schedules, or reporting minimum, maximum or prevailing wages. Oostendorp (2005) concludes that "the vast majority of the Inquiry statistics are non-comparable".

1.6 International Wage Trends since 1970

In this section we document international wage trends for the period 1970 to 2009 based on the UBS Prices and Earnings dataset. For the following charts we calculate a wage index for each city in which each profession is equally weighted. Thus, the charts reveal no distinctions by profession or skill level, but represent the average for the surveyed professions in manufacturing and services in each city. Cities for which no consumption-based PPP conversion rate is available in the Penn World Tables 7.1 are excluded.

Figure 1.1 plots the average annualised wage growth rate between 1970 and 2009 relative to the log 1970 wage. Only cities whose data both for 1970 and 2009 are available are included. The negative slope of the regression line suggest that this sample does not reject the hypothesis of absolute convergence in international wages for 1970-2009, as cities with lower initial real wages tended to have higher growth rates in average wages. This is somewhat in contrast to Morris (2009), who finds no indication of absolute convergence in manufacturing compensation costs for 1975-2006 in BLS data. Barro and Sala-i-Martin (2004) also find no evidence of absolute convergence in the more common convergence measure of GDP per capita for 1960-2000.

The clear outlier to the bottom of the chart is Mexico City, which experienced exceptionally weak real wage growth during this period. This is in line with Morris (2009), who also finds Mexico to be a distinct outlier in terms of its extremely weak real wage growth. The other three cities that experienced very weak wage growth during this period are Rio de Janeiro, Johannesburg and Sao Paulo, while the city that experienced the strongest wage growth is Hong Kong, in the upper left of the chart.

Instead of a linear regression we now fit a smoothed polynomial function to the data to identify potential trends when allowing for more variation in the slope, see figure 1.2. This display suggests the existence of at least two "clubs": A low to middle real wage club in which absolute convergence seems to prevail, and a high real wage club in which those with the highest initial wages grow faster. This high real wage club includes New York, Zurich, Chicago, Geneva, Amsterdam and Stockholm.

To verify whether the tentative evidence for absolute convergence is robust for a variation in start and end dates, we select nine sub-periods of different lengths and check the slopes on the linear trends: for all periods the slope is found to be negative, although for 1970-1988 and 1970-1991 the slope is not very steep. Figures 1.3 and 1.4 show examples of these results – here the total period is divided in half, resulting in the two periods 1970-1988 and 1988-2009: While the prior period shows limited evidence of absolute convergence, the

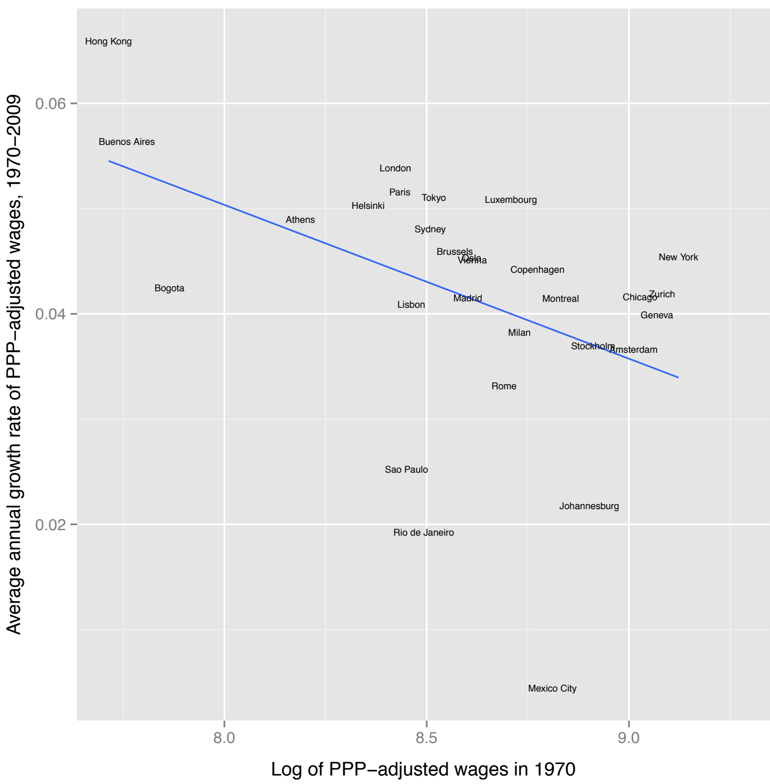


Figure 1.1: 1970-2009 average annualised wage growth rate relative to the log 1970 wage, and linear trend

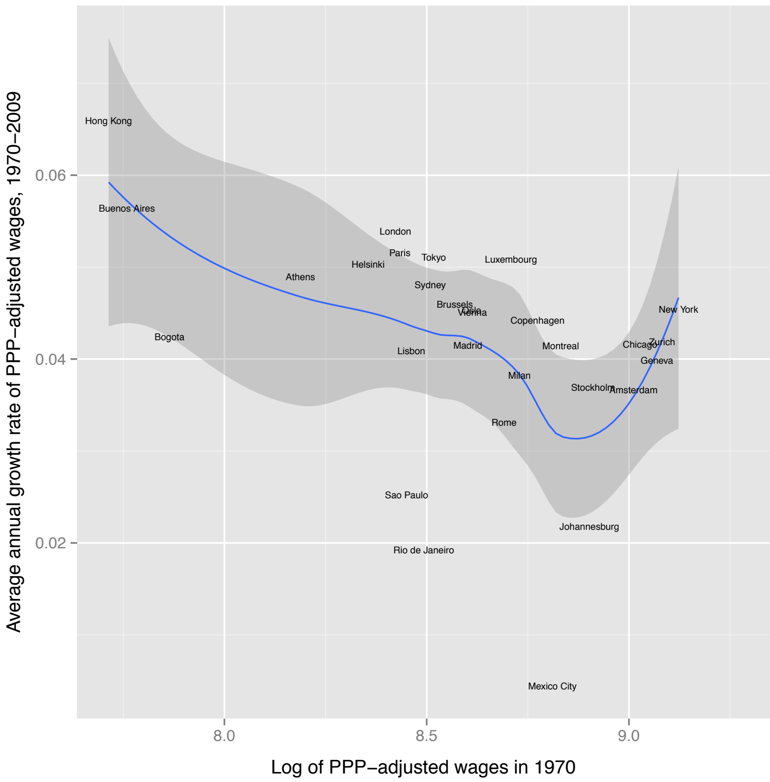


Figure 1.2: 1970-2009 average annualised wage growth rate relative to the log 1970 wage, and polynomial fit

latter period displays a steeper negative slope.[22] The two distinctly negative outliers for 1970-1988 in figure 1.3 are Mexico City and Rio de Janeiro, while the extreme positive outlier in 1988-2009 in figure 1.4 is Cairo, whose measured growth rate is likely distorted due to the massive currency devaluation that took place in 1988-1991, which is likely not reflected in the survey data as the survey took place from April to May 1988. The exclusion of these outliers does not, however, change the overall interpretation with regard to convergence.

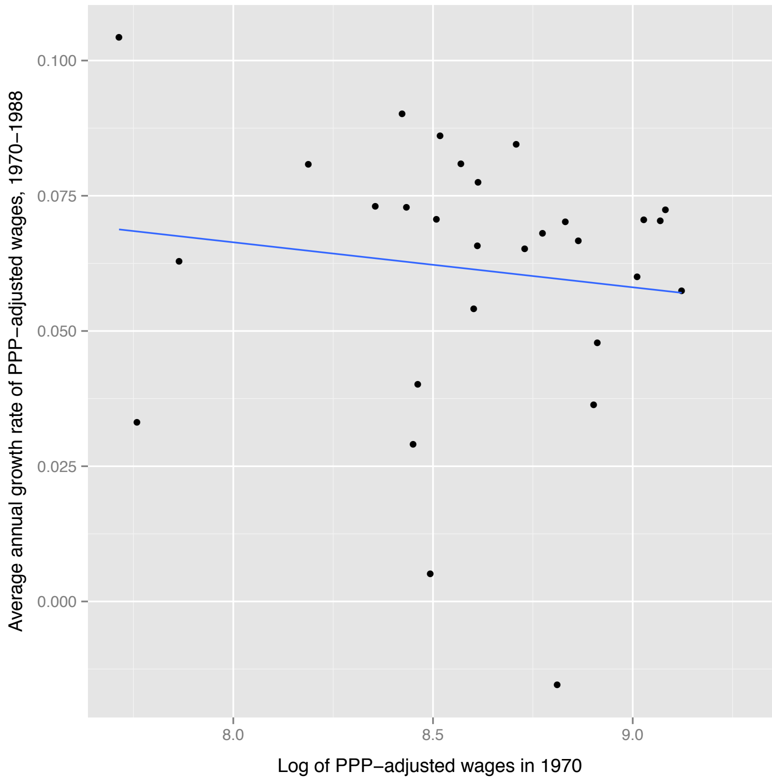


Figure 1.3: 1970-1988 average annualised wage growth rate relative to the log 1970 wage

Two further interpretations seem interesting when comparing these sub-periods: First, the diversity of growth rates is substantially higher for the earlier period 1970-1988. Second, the average level of growth rates is substantially lower for the later period 1988-2009. This points to a real wage growth slowdown. Figure 1.5 plots the average growth rates for 1970-1988 versus those for 1988-2009 and confirms a significant real wage growth slowdown

[22]Note that more cities could be included in the comparison for the second period, as data for more cities was available for 1988-2009 than for 1970-1988. City name labels have been removed in figures 1.3 and 1.4 for visual clarity.

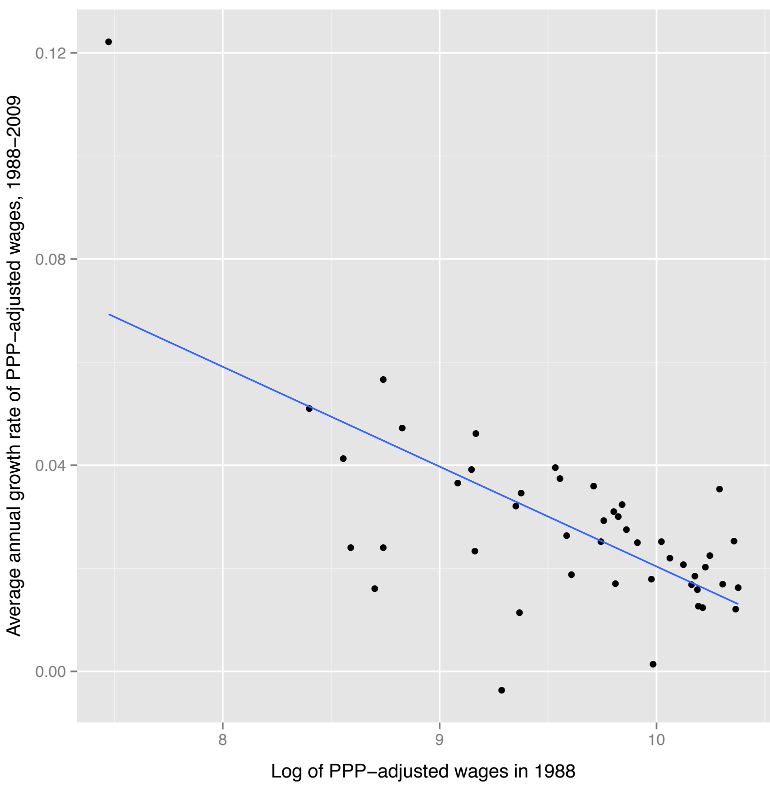


Figure 1.4: 1988-2009 average annualised wage growth rate relative to the log 1988 wage

between the earlier and later periods for the large majority of cities. The exceptions are the cities above the 45-degree line: Buenos Aires, Lisbon, Sao Paulo and Rio de Janeiro. This is in line with Durlauf et al. (2005) who records a significant slowdown in GDP per capita growth for the 1960-1980 versus 1980-2000 period in a similar chart.

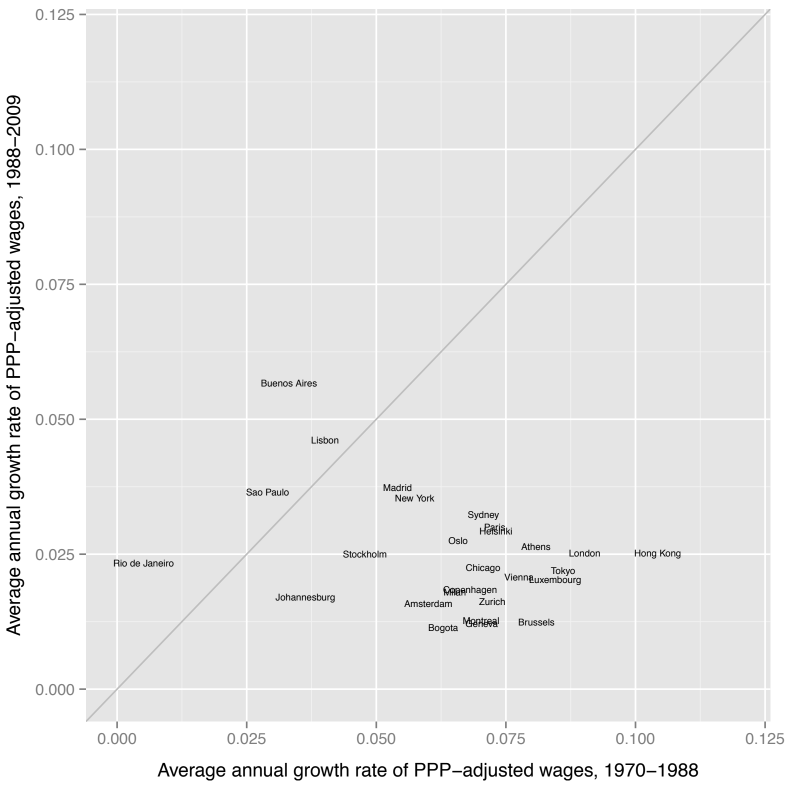


Figure 1.5: 1970-1988 wage growth rates versus 1988-2009 wage growth rates

Figure 1.5 includes only cities for which data is available from 1970. In contrast, figure 1.6 shows the kernel density distributions for three sub-periods, 1970-1985, 1985-2000 and 2000-2009, including all cities for which data are available for any sub-period. The leftward shift of the density distributions points to a decline in real wage growth rates from period to period.

While growth rates have declined, workers are nonetheless better off than in 1970. We use the average of real wages of the two American cities for which data is available in 1970, New York and Chicago, as a benchmark and compare all other cities' 1970 and 2009 real wages to this. The resulting kernel density distributions are shown in figure 1.7: For 1970 the distribution is single-peaked and nearly all cities have lower real wages than the 1970 US average. By 2009, the distribution has moved substantially to the right, with most

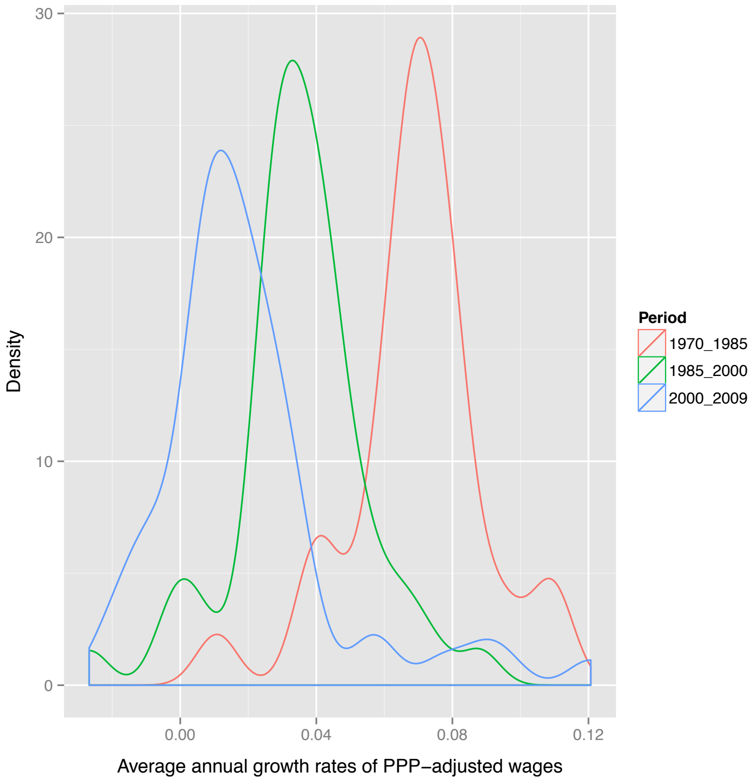


Figure 1.6: Densities of growth rates over different periods

cities now showing up real wages above the average US 1970 level. The distribution is also much more spread out in 2009 and displays the “Twin Peaks” often found in comparable GDP per capita data, see e.g. Durlauf et al. (2005). Note, that we have included all cities for which data in 2009 is available, thus the two distributions do not reflect the same base population. If we incorporate only the cities which are also included in the 1970 density, the left hand peak of the 2009 distribution flattens off, indicating that mostly lower-income cities have been added to the dataset since 1970.

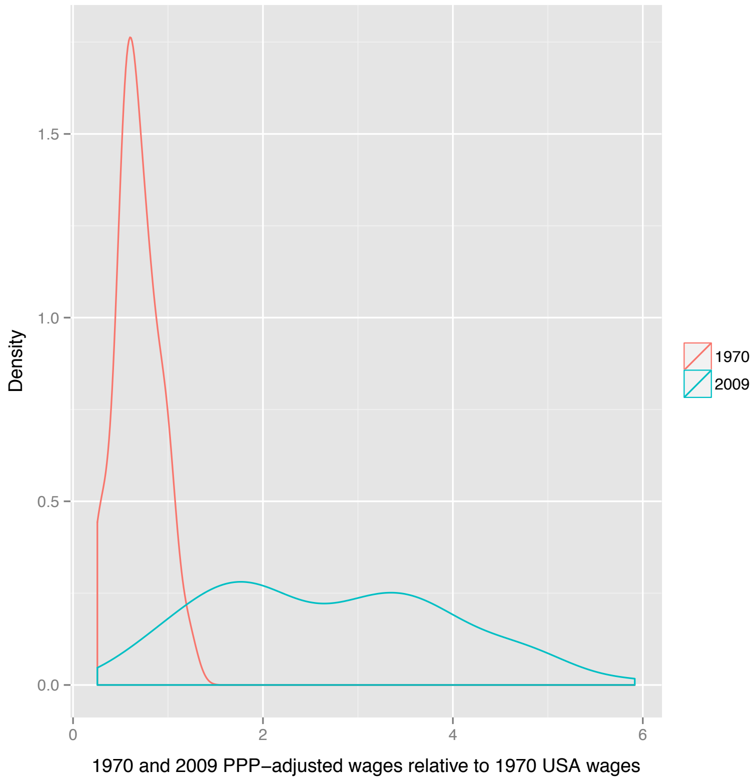


Figure 1.7: Densities of 1970 and 2009 real wages relative to 1970 US real wages

The two main inferences from the UBS Prices and Earnings wage data so far are that the hypothesis of absolute convergence is not rejected in a graphical sense in this data, and that real wage growth rates have slowed for most cities. Do the higher growth rates of cities with lower initial wages mean that they have been able to catch up to wealthier countries, or has the slowing of the group as a whole meant that the cities have mostly maintained their ranks relative to the rest of the group? Figure 1.8 takes the average of real wage growth in the American cities as the benchmark, and compares 1970 real wages for each city to the 1970 US average, and 2009 real wages for each city to the 2009 US average.[23] Cities below the 45-degree line were better positioned relative to the US in 1970 than in 2009 and thus have lost ground relative to the US, while the reverse is true for cities above the 45-degree line. Approximately as many cities are above as below the line. However, the clearest gains relative to US growth were made by cities with the lowest relative wages in 1970, which is visually reinforced by the slight tilt of the blue regression line relative to the 45-degree line.

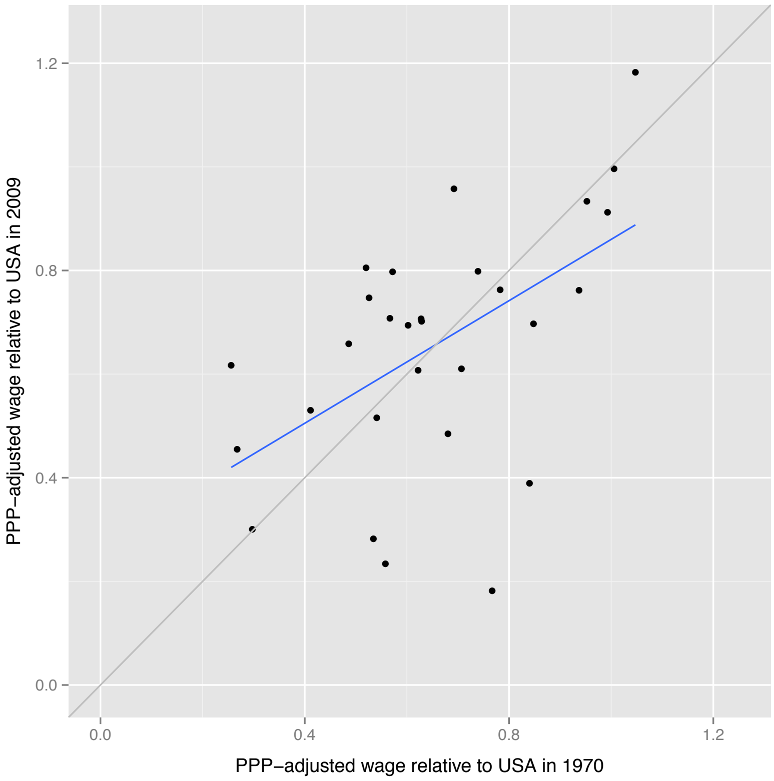


Figure 1.8: Change in the position of cities relative to the US average from 1970 to 2009

In many senses the wage data from the UBS Prices and Earnings surveys reflect what might be expected given past analyses for GDP per capita growth data over the same period: Wage growth rates have differed sharply between cities, but there is a clear trend of slowing real wage growth over the past 40 years, as has been seen for real GDP per capita. The density distribution of real wages has shifted clearly to the right, pointing to significant gains for workers, but it has also become two-peaked, meaning wage growth of the poorer countries was not sufficient over this time period to make up for the initial

[23]Cf. Durlauf et al. (2005) for a similar comparison using GDP per capita. City name labels have been excluded for visual clarity.

difference in levels. One aspect in which these data differ from much of the GDP per capita data is that in a graphical sense they do not reject the hypothesis of absolute convergence in real wages. In the next section we analyse the question of absolute and conditional convergence from a statistical perspective.

1.7 Econometric Model Specification and Estimation

From the neoclassical model we reduce equation (1.36) to the form

$$\frac{1}{t} \cdot \ln \left(\frac{w(t)}{w(0)} \right) = a + b \cdot \ln w(0) + c' \cdot x + d' \cdot z + \epsilon \tag{1.38}$$

where the logarithm of the growth rate of wages is a function of the logarithm of the initial wage rate $\ln w(0)$, a vector x of the variables that determine the steady state in the Solow-Swan model, and a vector z of other variables. Within the Solow-Swan model z may be interpreted as including not only the starting level of technology, but also environmental factors such as resource endowments, the quality of public institutions, climate, etc., equivalent to the interpretation of $A(0)$ by Mankiw et al. (1992): In this context these variables can be interpreted as additional measures to ensure that one is controlling sufficiently for the steady state when testing for conditional convergence. Separately, in the context of endogenous growth models, these additional variables would generally be interpreted as stand-alone drivers of growth. In the following sections we take a negative coefficient b on the initial wage regressor $lnw(0)$ to signify convergence in wages. This is an analog approach to the analyses of growth convergence, where a negative coefficient on the initial level of GDP per capita signals growth convergence.

1.7.1 Absolute Convergence in Mean Wages

In a first simple step, we verify the hypothesis of absolute convergence for average city wages for the period 1970-2009, including the 29 cities for which growth of the mean wage for 1970-2009 could be calculated from the data. The results of the OLS regression are shown in column (1) of table 1.1, with the standard errors shown in brackets. The convergence coefficient b on the log mean wage in the starting year 1970 is negative and significant at the 5% level, indicating absolute convergence in mean wages across cities.

As the graphical representations in the previous section indicated that wage growth developed differently over the course of the nearly 40 years under observation, we test the absolute convergence hypothesis for the sub-periods 1970-1988 and 1988-2009. The OLS regression results from separately testing these periods are shown in column (2) of table

1.1 for the balanced panel, i.e. when including only the 28 cities for which both growth rates 1970-1988 and 1988-2009 can be calculated.[24] Column (3) shows the results for the unbalanced panel, i.e. when including in the 1970-1988 regression all 29 cities for which 1970-1988 data is available,[25] and in the 1988-2009 regression all 46 cities for which this data is available. For both sub-periods the regression coefficients b on the initial mean wage are negative, however, absolute convergence seems stronger for 1988 to 2009, as for this period the absolute value of the coefficient on the initial wage is larger, the R^2 is higher and the regression is significant at the 0.1% level, as compared to the 5% level for the total period 1970-2009, and no statistical significance for the 1970-1988 period.

Table 1.1: Average city wage growth regressed on initial average wages (absolute convergence) for 1970-2009, and the sub-periods 1970-1988 and 1988-2009

	(1) OLS 1970-2009 Total period (balanced) (unrestricted)	(2) OLS 1970-1988 1988-2009 Estimated separately (balanced) (unrestricted)	(3) OLS 1970-1988 1988-2009 Estimated separately (unbalanced) (unrestricted)	(4) SUR 1970-1988 1988-2009 (balanced) (unrestricted)	(5) SUR 1970-1988 1988-2009 (unbalanced) (unrestricted)	(6) SUR 1970-1988 1988-2009 (balanced) (restricted)	(7) SUR 1970-1988 1988-2009 (unbalanced) (restricted)
Intercept 1	0.161 (0.047)**	0.129 (0.110)	0.125 (0.109)	0.134 (0.109)	0.149 (0.106)	0.181 (0.026)***	0.221 (0.016)***
Ln average wage 1970	-0.014 (0.006)*	-0.008 (0.013)	-0.008 (0.013)	-0.009 (0.013)	-0.011 (0.012)	-0.014 (0.003)***	-0.019 (0.002)***
Intercept 2		0.174 (0.030)***	0.204 (0.018)***	0.185 (0.030)***	0.214 (0.017)***	0.164 (0.029)***	0.209 (0.017)***
Ln average wage 1988		-0.015 (0.003)***	-0.018 (0.002)***	-0.016 (0.003)***	-0.019 (0.002)***	-0.014 (0.003)***	-0.019 (0.002)***
Multiple R^2	0.19	0.02/0.49	0.01/0.44	0.02/0.49	0.01/0.44	0.01/0.49	-0.02/0.44
Adjusted R^2	0.16	-0.02/0.47	-0.02/0.43	-0.02/0.47	-0.03/0.43	-0.03/0.47	-0.05/0.43
Num. obs.	29	28/28	29/46	28/28	29/46	28/28	29/46

Notes: The term "balanced" indicates that only cities were included, for which data was available for both periods 1970-1988 and 1988-2009, or for the total period 1970-2009. In the "unbalanced" panels additional cities, for which data was only available in one of the periods were added to benefit from the larger sample size. The term "restricted" indicates that the convergence coefficients b on the initial wage were restricted to be equal in both periods, while the intercepts for the two periods were allowed to vary. The standard errors are shown in brackets. In the lower section of the table where two numbers are shown separated by a slash, the first number refers to 1970-1988, while the second number refers to 1988-2009.

$^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$, $p < 0.1$

We can use the panel structure of the data to improve the efficiency of the parameter estimates and to test whether the coefficients on the initial wage for the two periods are statistically different from one another. For this we set the two sub-periods as the cross section $i \in$ (1970-1988; 1988-2009), while the cities c represent the second panel dimension.

$$\ln\left(cagr\_w_{i,c}\right) = a_i + b_i \cdot \ln w(0)_{i,c} + \epsilon \tag{1.39}$$

For each period i the logarithm of the compound annual growth rate of the average wage in each city $\ln\left(cagr\_w_{i,c}\right)$ is regressed on the logarithm of the initial average wage $\ln w(0)_{i,c}$. In the Seemingly Unrelated Regressions (SUR) methodology the Feasible Generalised Least Squares (FGLS) estimator uses the correlation within the panel to improve the efficiency

[24] Compared to the 1970-2009 period, Rome was omitted as it was not included in the 1988 survey.
[25] Düsseldorf, for which data is available in 1970 and 1988, but not in 2009, is added.

of the OLS estimates. We aim to identify differences between the sub-periods, and we can plausibly assume that the regressions between cities within each period are in some way correlated. However, we cannot exclude that they are also correlated across periods.

The results of the SUR regressions are shown in column (4) of table 1.1 for the balanced panel and in column (5) for the unbalanced panel. In both cases and for both periods the convergence coefficient b increased slightly compared to the equivalent OLS regressions. It is still higher for the 1988-2009 period as compared to the earlier period. However, when we test the hypothesis of identical convergence coefficients for the two periods, $b_{i=1970-1988} = b_{i=1988-2009}$, it is not rejected at any reasonable level of significance by Theil's F-Test, nor by the Chi-squared statistic or F-statistic of a Wald test. Thus, we restrict the convergence coefficient b to be identical across both periods (while allowing the intercept to vary), and repeat the SUR estimation. The results for the balanced panel are shown in column (6) of table 1.1 while the results of the unbalanced panel are shown in column (7). All coefficients are significant at the 0.1% level.

The different estimates in table 1.1 overall confirm the hypothesis of absolute convergence in mean city wages. We subsequently deduce the speed of convergence that these estimates imply. From equation (1.36) it follows that the coefficient $b = -\frac{(1-e^{-\sigma_w t})}{t}$, so the approximate speed of convergence in the neighbourhood of the steady state can be computed as $\sigma_w = -\frac{\ln(1+t \cdot b)}{t}$, giving a speed of convergence of 0.8% to 1.5% per annum for 1970-2009 as a whole and for 1988-2009, but only of about 0.4% to 0.5% for 1970-1988 when estimated separately.

Thus far we have employed only the city mean wage data - next we turn to the richer profession-level wage data in the UBS Prices and Earnings surveys. We structure the following regression analysis in four parts: First, we again test for absolute convergence (section 1.7.2). Second, we test for conditional convergence (section 1.7.3) in two senses: 1) within more homogenous groups by separately testing cities in emerging and developed markets for absolute convergence, and 2) in the sense of the Solow-Swan model by adding regressors that control for the steady state (see the interpretation of x in equation (1.38)). We also make use of the skill attributions to provide insight into the question of how different skill and professional groups have been affected by the international wage trends implied by conditional convergence. Finally, we use principal components augmented regressions to explain the impact of additional explanatory variables that go beyond the Solow-Swan model (see z in equation (1.38)), in line with the Barro-type equations of growth regressions (section 1.7.4).

1.7.2 Absolute Convergence in Profession-Level Wages

Absolute Convergence in Profession-Level Wages and Comparison of Sub-periods 1970-1988 and 1988-2009

Table 1.2 reports the results when regressing wage growth on initial wages using profession-level wage data for the periods 1970-2009, 1970-1988 and 1988-2009. The OLS results are shown in columns (1) to (3). For the SUR regressions in columns (4) to (7) we use the equivalent panel structure to that used for the mean wage regressions in the previous section:

$$\ln\left(cagr\_w_{i,pc}\right) = a_i + b_i \cdot \ln w(0)_{i,pc} + \epsilon \tag{1.40}$$

Thus, for each period $i \in$ (1970-1988; 1988-2009), which again represents the cross section, the logarithm of the compound annual growth rate of the wage of each profession in each city $\ln\left(cagr\_w_{i,pc}\right)$ is regressed on the logarithm of the initial wage $\ln w(0)_{i,pc}$ of that profession in that city (pc).

Table 1.2: Profession-level wage growth regressed on initial profession-level wages (absolute convergence) for 1970-2009, and the sub-periods 1970-1988 and 1988-2009

	(1) OLS 1970-2009 Total period (balanced) (unrestricted)	(2) OLS 1970-1988 1988-2009 Estimated separately (balanced) (unrestricted)	(3) OLS 1970-1988 1988-2009 Estimated separately (unbalanced) (unrestricted)	(4) SUR 1970-1988 1988-2009 (balanced) (unrestricted)	(5) SUR 1970-1988 1988-2009 (unbalanced) (unrestricted)	(6) SUR 1970-1988 1988-2009 (balanced) (restricted)	(7) SUR 1970-1988 1988-2009 (unbalanced) (restricted)
Intercept 1	0.157 (0.018)***	0.183(0.042)***	0.179 (0.041)***	0.181 (0.042)***	0.178 (0.042)***	0.188 (0.017)***	0.180 (0.006)***
Ln wage 1970	-0.013 (0.002)***	-0.014 (0.005)**	-0.014 (0.005)**	-0.014 (0.005)**	-0.014 (0.005)**	-0.015 (0.002)***	-0.014 (0.001)***
Intercept 2		0.159 (0.020)***	0.158 (0.009)***	0.168 (0.020)***	0.159 (0.007)***	0.168 (0.019)***	0.159 (0.007)***
Ln wage 1988		-0.014 (0.002)***	-0.014 (0.001)***	-0.015 (0.002)***	-0.014 (0.001)***	-0.015 (0.002)***	-0.014 (0.001)***
Multiple R^2	0.22	0.06/0.24	0.06/0.29	0.06/0.24	0.06/0.29	0.06/0.24	0.06/0.29
Adjusted R^2	0.22	0.05/0.24	0.05/0.29	0.05/0.24	0.05/0.29	0.05/0.24	0.05/0.29
Num. obs.	144	136/136	141/539	136/136	141/539	136/136	141/539

Notes: The term "balanced" indicates that only cities were included, for which data was available for both periods 1970-1988 and 1988-2009, or for the total period 1970-2009. In the "unbalanced" panels additional cities, for which data was only available in one of the periods were added to benefit from the larger sample size. The term "restricted" indicates that the convergence coefficients b on the initial wage were restricted to be equal in both periods, while the intercepts for the two periods were allowed to vary. The standard errors are shown in brackets. In the lower section of the table where two numbers are shown separated by a slash, the first number refers to 1970-1988, while the second number refers to 1988-2009.
$^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$, $p < 0.1$

As in the previous section, the convergence coefficients b are again all negative, but they are more similar across time periods, all close to -0.014. Statistical significance is high for all specifications; at its lowest it is still at the 1% level. Explanatory power is, as in the mean wage regressions of the previous section, again lower for the 1970-1988 period. The hypothesis of identical convergence coefficients $b_{i=1970-1988} = b_{i=1988-2009}$ in the two periods is again not rejected at any reasonable level of significance by Theil's

F-Test, nor by the Chi-squared statistic or F-statistic of a Wald test. Thus, we again restrict the convergence coefficient b to be identical across both periods (while allowing the intercept to vary), and repeat the SUR estimation, with the results shown in column (6) for the balanced panel and column (7) for the unbalanced panel of table 1.2. Again, all the coefficients of the restricted equations are significant at the 0.1% level.

Overall these results confirm the hypothesis of absolute convergence in profession-level wages, with a convergence speed of 0.8%-1.0% per annum. Interestingly, the results overall are relatively similar to those shown for the more aggregated mean wage data in table 1.1. This was not necessarily to be expected, as the convergence measured using mean wages is structurally different from that measured when using profession-level data: The former measures only convergence between cities with lower and higher average initial wages, while the latter measures convergence of lower-paid jobs relative to higher paid jobs, across the world. This is independent of whether the level of wages in a job is due to the city, or the profession.

Absolute Convergence in Profession-Level Wages based on GDP per Capita

A specification that one might consider more comparable to the mean wage specification of section 1.7.1 is one in which profession-level wage growth rates are regressed on the countries' initial PPP-adjusted GDP per capita. Thus, the initial wage is replaced by the initial level of GDP per capita as the initial condition. This specification is *not* the kind of convergence we derived for wages in the Solow-Swan model, nor is it the type of convergence that follows from the migration or trade frameworks. Nonetheless, as initial GDP per capita is the common variable used as the initial condition when analysing economic growth convergence, we investigate this relationship also. Table 1.3 shows some selected specifications of such equations.[26]

Column (1) in table 1.3 shows the OLS estimation over the total period 1970-2009, while column (2) shows the equivalent for the sub-periods 1970-1988 and 1988-2009. We additionally ran a number of regressions controlling for several fixed effects (city, country, regions, skill levels and professions) and combinations of these, testing these specifications both on the total period and the two sub-periods. None of the tested specifications showed strong evidence for absolute convergence, as convergence coefficients were found to be positive in some periods, or to be statistically insignificant. The remaining columns (3) to (6) show two examples of this type of specification: One with only city fixed effects (in columns (3) for the total period, and column (4) for the sub-periods), whose results

[26]The source of PPP-adjusted GDP per capita data is the Penn World Tables 7.1.

Table 1.3: Profession-level wage growth regressed on initial real GDP per capita for 1970-2009, and the sub-periods 1970-1988 and 1988-2009

	(1) OLS 1970-2009 Total period (balanced)	(2) OLS 1970-1988 1988-2009 Estimated separately (unbalanced)	(3) OLS + Fixed effects (City) 1970-2009 Total period (balanced)	(4) OLS + Fixed effects (City) 1970-1988 1988-2009 Estimated separately (unbalanced)	(5) OLS + Fixed effects (Skills, Regions) 1970-2009 Total period (balanced)	(6) OLS + Fixed effects (Skills, Regions) 1970-1988 1988-2009 Estimated separately (unbalanced)
Intercept 1	-0.015 (0.017)	-0.113 (0.032)***	-0.065 (0.108)	-0.212 (0.205)	0.064 (0.026)*	0.081 (0.048).
Ln GDP/capita 1970	0.006 (0.002)***	-0.018 (0.003)***	0.010 (0.011)	0.028 (0.020)	-0.005 (0.003)	-0.004 (0.006)
factor(South-East Asia)1 1					0.037 (0.006)***	0.057 (0.011)***
factor(Europe)1 1					0.025 (0.006)***	0.032 (0.011)**
factor(Latin America)1 1					0.006 (0.005)	-0.012 (0.009)
factor(Skills)Level2 1					0.002 (0.002)	-0.006 (0.004)
factor(Skills)Level3 1					0.004 (0.003)	-0.003 (0.005)
Intercept 2		0.085 (0.009)***		-0.016 (0.282)		0.084 (0.013)***
Ln GDP/capita 2		-0.006 (0.001)***		0.003 (0.027)		-0.006 (0.001)***
factor(South-East Asia)1 2						-0.001 (0.003)
factor(Europe)1 2						-0.002 (0.003)
factor(Latin America)1 2						-0.005 (0.003)
factor(Skills)Level2 2						0.002 (0.002)
factor(Skills)Level3 2						-0.002 (0.002)
Multiple R^2	0.08	0.18/0.06	0.76	0.79/0.48	0.35	0.46/0.07
Adjusted R^2	0.07	0.17/0.06	0.70	0.74/0.43	0.33	0.44/0.06
Num. obs.	144	141/539	144	141/539	144	141/539

Notes: The term "balanced" indicates that only cities were included, for which data was available for both periods 1970-1988 and 1988-2009, or for the total period 1970-2009. In the "unbalanced" panels additional cities, for which data was only available in one of the periods were added to benefit from the larger sample size. The standard errors are shown in brackets. In the lower section of the table where two numbers are shown separated by a slash, the first number refers to 1970-1988, while the second number refers to 1988-2009. The coefficients on the city dummy variables in columns (3) and (4) are omitted in this table for brevity, but the complete columns (3) and (4) can be found in table B.1 in the appendix.
$^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$, $p < 0.1$

do not point to convergence, and one with region dummies for South-East Asia, Europe, Latin America and the professions' skill levels (in columns (5) and (6)), whose convergence coefficients are negative, but rather small, with statistical significance only for 1988-2009.[27]

Overall, the regressions in which profession-level wage growth rates are regressed on the countries' initial PPP-adjusted GDP per capita do not point to absolute convergence, with regressor estimates changing sign for different periods, and different specifications. This is in contrast to the results of the mean wage specification of section 1.7.1 and implies that initial GDP per capita and initial mean wages are not substitutable in these regressions. However, this result is in line with Stone et al. (2011) who also regressed wage growth on initial GDP per capita and found no systematic evidence of convergence.

Summarising the results for absolute convergence in profession-level data so far, we find no statistical evidence of catch-up in wages for poorer countries as measured by

[27] The coefficients on the city dummy variables in columns (3) and (4) are omitted in this table for brevity. These are reproduced in table B.1 in the appendix.

GDP per capita, but we find solid evidence of absolute convergence in two senses: Cities with lower *average* initial wages exhibit higher growth in *average wages*; and lower initial *profession-level wages* world wide (whether due to the country context or the profession) also exhibit higher growth.

Absolute Convergence in Profession-Level Wages for Skill Levels and Professions

Having established solid evidence for absolute convergence when average city wages and profession-level wages are regressed on initial wages, we further investigate the characteristics of convergence within skill levels and professions.

First, we simultaneously estimate equations for each skill level in a SUR model in which we take the skill levels s as the cross section, and the cities c as the second panel dimension:

$$\ln\left(cagr\_w_{s,c}\right) = a_s + b_s \cdot \ln w(0)_{s,c} + \epsilon \tag{1.41}$$

As the number of professions and cities surveyed in 1970 is lower than in later years, we again analyse the data for the whole period 1970-2009 (see column (1) in table 1.4), but also divide the data into the two sub-periods 1970-1988 and 1988-2009, providing a richer dataset (see columns (3) and (4) in table 1.4). Note that the data is now pooled in the sense that the model does not distinguish between the two periods when we regress the compound annual growth rate of profession-level wages on initial wages for each skill level. Columns (3) and (4) in table 1.4 show the results for the balanced and unbalanced panels of the subdivided data.[28]

Again, we find solid evidence of absolute convergence, this time within the separate skill levels of professions with convergence coefficients systematically having a negative sign, and high statistical significance. This means that across cities the lowest wages within each skill group have been catching up to the higher wages within the same skill group. However, the implied convergence speeds differ, depending on the specification, and no skill level has a systematically faster or slower convergence speed. The hypothesis of identical convergence coefficients $b_{s=skill\ level\ 1} = b_{s=skill\ level\ 2} = b_{s=skill\ level\ 3}$ across skill levels is not rejected when using the data over the total period 1970-2009, but is rejected at the 5% level or better for the sub-period data in columns (3) and (4) by Theil's F-Test, the Chi-squared statistic and the F-statistic of a Wald test. Thus, we restrict the convergence coefficient b_s to be identical across skill levels only for the total period 1970-2009 (while

[28] The balanced dataset has been set up so that it includes only cities for which data for each skill level is available, and also, that only professions were included for which data in both periods are available.

Table 1.4: Absolute convergence in profession-level wages for skill levels 1, 2 and 3

	(1) SUR 1970-2009 (unbalanced) (unrestricted)	(2) SUR 1970-2009 (unbalanced) (restricted)	(3) SUR 1970-1988 1988-2009 (balanced) (unrestricted)	(4) SUR 1970-1988 1988-2009 (unbalanced) (unrestricted)
Skill level 1 - Intercept	0.183 (0.020)***	0.166 (0.013)***	0.126 (0.040)**	0.263 (0.025)***
Skill level 1 - Ln initial wage	-0.017 (0.002)***	-0.015 (0.002)***	-0.011 (0.004)*	-0.026 (0.003)***
Skill level 2 - Intercept	0.156 (0.020)***	0.167 (0.013)***	0.161 (0.042)***	0.200 (0.038)***
Skill level 2 - Ln initial wage	-0.013 (0.002)***	-0.015 (0.002)***	-0.014 (0.004)**	-0.019 (0.004)***
Skill level 3 - Intercept	0.171 (0.021)***	0.171 (0.013)***	0.249 (0.028)***	0.184 (0.021)***
Skill level 3 - Ln initial wage	-0.015 (0.002)***	-0.015 (0.002)***	-0.023 (0.003)***	-0.016 (0.002)***
Multiple R^2	0.06/0.38/0.08	0.09/ 0.40/0.08	-0.11/0.16/0.38	0.35/0.15/0.37
Adjusted R^2	0.02/0.36/0.04	0.05/ 0.37/0.04	-0.16/0.12/0.36	0.34/0.13/0.36
Num. obs.	28/29/29	28/29/29	27/27/27	47/47/46

Notes: The term "balanced" indicates that only cities were included, for which data was available for all skill levels, and only professions were included, for which data in both periods 1970-1988 and 1988-2009 were available. In the "unbalanced" panels additional cities, for which data was only available for some of the skill levels or one of the periods were added to benefit from the larger sample size. The term "restricted" indicates that the convergence coefficients b on the initial wage were restricted to be equal in all skill groups, while the intercepts for the skill groups were allowed to vary. The standard errors are shown in brackets. In the lower section of the table where three numbers are shown separated by a slash, the first number refers to skill level 1, the second to skill level 2, and the third to skill level 3.

$^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$, $p < 0.1$

allowing the intercept to vary), and repeat the SUR estimation, with the results shown in column (2) of table 1.4. Again, the convergence coefficient is negative; it points to a convergence speed of 1% per annum, and all the coefficients of the restricted equations are significant at the 0.1% level.

Next, we use the panel structure in which the professions p represent the cross section and the cities c represent the second panel dimension:

$$\ln\left(cagr\_w_{p,c}\right) = a_p + b_p \cdot \ln w(0)_{p,c} + \epsilon \tag{1.42}$$

Again we analyse the data for the whole period 1970-2009 (see column (1) in table 1.5), but also divide the data into the two sub-periods 1970-1988 and 1988-2009 (see columns (3) and (4) in table 1.5). Again, the data is pooled in the sense that the model does not distinguish between the two sub-periods when we regress the compound annual growth rate of profession-level wages on initial wages for each profession.[29]

Again, we find solid evidence of absolute convergence, this time within the professions with convergence coefficients systematically having a negative sign, and high statistical

[29] The balanced dataset has been set up so that it includes only cities for which data for each profession is available, and also, that only professions were included for which data in both periods are available.

Table 1.5: Absolute convergence in profession-level wages for the different professions

	(1) SUR 1970-2009 (unbalanced) (unrestricted)	(2) SUR 1970-2009 (unbalanced) (restricted)	(3) SUR 1970-1988 1988-2009 (balanced) (unrestricted)	(4) SUR 1970-1988 1988-2009 (unbalanced) (unrestricted)
Auto.Mechanics - Intercept	0.206 (0.021)***	0.188 (0.012)***	0.180 (0.037)***	0.231 (0.020)***
Auto.Mechanics - Ln initial wage	-0.020 (0.002)***	-0.018 (0.001)***	-0.016 (0.004)***	-0.022 (0.002)***
Bank.Tellers.or.Credit.Clerks - Intercept	0.240 (0.023)***	0.196 (0.012)***	0.288 (0.034)***	0.329 (0.027)***
Bank.Tellers.or.Credit.Clerks - Ln initial wage	-0.023 (0.003)***	-0.018 (0.001)***	-0.026 (0.004)***	-0.030 (0.003)***
Bus.Drivers - Intercept	0.183 (0.019)***	0.189 (0.012)***	0.179 (0.037)***	0.247 (0.021)***
Bus.Drivers - Ln initial wage	-0.017 (0.002)***	-0.018 (0.001)***	-0.016 (0.004)***	-0.023 (0.002)***
Construction.Workers - Intercept				0.216 (0.018)***
Construction.Workers - Ln initial wage				-0.021 (0.002)***
Cooks - Intercept				0.424 (0.024)***
Cooks - Ln initial wage				-0.040 (0.002)***
Department.Managers - Intercept				0.452 (0.022)***
Department.Managers - Ln initial wage				-0.041 (0.002)***
Electrical.Engineers - Intercept				0.348 (0.029)***
Electrical.Engineers - Ln initial wage				-0.032 (0.003)***
Factory.or.Textile.Workers - Intercept				0.217 (0.022)***
Factory.or.Textile.Workers - Ln initial wage				-0.021 (0.002)***
Industrial.Workers - Intercept				0.370 (0.026)***
Industrial.Workers - Ln initial wage				-0.035 (0.003)***
Primary.School.Teachers - Intercept	0.184 (0.019)***	0.194 (0.012)***	0.265 (0.028)***	0.208 (0.019)***
Primary.School.Teachers - Ln initial wage	-0.016 (0.002)***	-0.018 (0.001)***	-0.024 (0.003)***	-0.019 (0.002)***
Saleswomen - Intercept				0.304 (0.017)***
Saleswomen - Ln initial wage				-0.030 (0.002)***
Secretaries - Intercept	0.217 (0.021)***	0.191 (0.012)***	0.173 (0.042)***	0.279 (0.025)***
Secretaries - Ln initial wage	-0.021 (0.002)***	-0.018 (0.001)***	-0.015 (0.004)**	-0.027 (0.003)***
Multiple R^2	0.23/0.36/0.06/0.06/0.55	0.25/0.38/0.04/0.03/0.52	0.10/0.52/-0.34/0.35/0.13	See note 1
Adjusted R^2	0.20/0.33/0.02/0.02/0.53	0.22/ 0.35/0.00/0.00/0.50	0.07/0.50/-0.39/0.32/0.10	See note 2
Num. obs.	29/29/28/29/29	29/29/28/29/29	27/27/27/27/27	See note 3

Note 1: 0.31/0.18/0.06/-0.27/0.66/0.58/0.21/-0.13/0.34/0.31/0.23/0.00

Note 2: 0.30/0.16/0.04/-0.30/0.65/0.57/0.20/-0.16/0.33/0.29/0.21/-0.03

Note 3: 46/47/47/45/45/45/45/43/44/46/45/46

Additional notes: The term “balanced” indicates that only cities were included, for which data was available for all professions, and only professions were included, for which data in both periods 1970-1988 and 1988-2009 were available. In the “unbalanced” panels additional cities, for which data was only available for some of the professions or one of the periods were added to benefit from the larger sample size. The term “restricted” indicates that the convergence coefficients b on the initial wage were restricted to be equal in all professions, while the intercepts for the professions were allowed to vary. The standard errors are shown in brackets. In the lower section of the table where numbers are shown separated by a slash, the numbers refer to the professions in alphabetical order, as included in that specific regression.

$^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$, $p < 0.1$

significance. This means that across cities but within each profession the lowest wages have been catching up to the higher wages within the same profession. The unbalanced panel (see column (4) in table 1.5), which takes into account both sub-periods 1970-1988 and 1988-2009, and includes more professions, points to slightly higher speeds of convergence than the equivalent balanced panel and the regressions using only data for the whole 1970-2009 period.

The hypothesis of identical convergence coefficients b_p across professions is again not rejected when using the data over the total period 1970-2009, but is rejected at the 5% level or better for the sub-period data in columns (3) and (4) by Theil’s F-Test, the

Chi-squared statistic and the F-statistic of a Wald test. Thus, we restrict the convergence coefficient b_p to be identical across professions only for the total period 1970-2009 (while allowing the intercept to vary), and repeat the SUR estimation, with the results shown in column (2) of table 1.5. Again, the convergence coefficient in this restricted regression is negative and all estimates are significant at the 0.1% level. This specification points to a convergence speed within professions of about 1.4% per annum, which is slightly faster than the convergence speed within skill groups of 1% per annum. This again is slightly faster than the convergence speed of 0.8%-1.0% found at the beginning of section 1.7.2 where all professions are grouped together. This is a plausible result as convergence forces are likely to be strongest within relatively homogenous groups such as individual professions, less strong in the somewhat less homogenous skill levels, and weakest when including all professions.

1.7.3 Conditional Convergence in Profession-Level Wages

Convergence in Emerging / Developed Markets for Profession-Level Wages

In this section we use a very simple form of conditioning, we divide the wage data into two distinct groups: wages from emerging markets and wages from developed markets. We subsequently test for absolute convergence within these distinct and somewhat more homogenous groups. The results from the OLS regressions are shown in table 1.6 with column (1) repeating the equivalent results for all data from table 1.2, and column (2) and (3) providing the new estimates for developed and emerging markets separately.

The results point to convergence within both groups, but convergence seems stronger within the emerging markets, where the absolute value of the coefficient on the initial wage has risen significantly relative to the full sample of countries, and the adjusted R-squared too is now higher at 0.64. However, also for developed markets explanatory power is higher now at 0.33 as compared to 0.22 for the equivalent regression in the full sample. Both regressions are significant at the 0.1% level. Overall, this simple type of conditioning points to potentially somewhat stronger convergence within the more homogenous groups of cities. It must be noted, however, that the sample size for emerging markets over the time period 1970-2009 is rather small and is potentially not representative of trends in all emerging markets over this period.

Solow-Swan Conditional Convergence in Profession-Level Wages

We test for conditional convergence in the Solow-Swan sense, by introducing the vector x of variables that define the steady state in the Solow-Swan model. To check the robustness

Table 1.6: Absolute convergence in developed markets and emerging markets

	(1) OLS 1970-2009 (balanced) All cities	(2) OLS 1970-2009 (balanced) Developed markets	(3) OLS 1970-2009 (balanced) Emerging markets
Intercept	0.157 (0.018)***	0.158 (0.015)***	0.272 (0.032)***
Ln wage 1970	-0.013 (0.002)***	-0.013 (0.002)***	-0.029 (0.004)***
Multiple R^2	0.22	0.34	0.64
Adjusted R^2	0.22	0.33	0.63
Num. obs.	144	110	34

Notes: The term "balanced" indicates that only cities were included, for which data was available for the total period 1970-2009. The standard errors are shown in brackets. $^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$, $p < 0.1$

of the results, two separate datasets are used. The first dataset "SDM" is drawn from Sala-i-Martin et al. (2004), and includes data mainly from the 1960s and 1970s in a cross section. Initially we include the average population growth rate for each country for 1960-1990 and the average fertility rate in the 1960s as proxies for population growth. Public spending on education as a share of GDP in the 1960s, levels of higher education in 1960, as well as life expectancy in 1960 in each country are investigated as proxies for savings in human capital. Savings in physical capital are proxied by public investment as a share of GDP[30] and the investment share of GDP per capita, with the last variable stemming from the Penn World Tables 7.1. However, only the log initial wage level and the fertility rate are statistically significant. We therefore keep these regressors and verify if the model fit can be improved by adding further regressors in a stepwise process. We find that adding only the higher education variable (H60) provides for a good model fit, as shown in column (1) of table 1.7. The fertility rate is significant at the 0.1% level, while higher education is significant at the 5% level. The absolute value of the coefficient on the initial wage has increased to 0.02, pointing to a convergence speed σ_w of 1.7% per annum, significantly faster than the unconditional rate of convergence for the full sample and equivalent time period.

The second dataset we use is a panel dataset comprising variables from the World

[30] The level of public investments as a share of GDP for Switzerland, which is missing in the original SDM dataset, was calculated for 1970 from the tables U.17, U.24, U.27 and Q.2 of the Research Center for Social and Economic History of the University of Zurich, provided at http://www.fsw.uzh.ch/hstat/nls/overview.php.

Table 1.7: Profession-level wage growth regressed on initial profession-level wages and additional variables that define the Solow-Swan steady state (conditional convergence) for 1970-2009, and the sub-periods 1970-1988 and 1988-2009

	(1) OLS 1970-2009 Total period (balanced) (unrestricted) SDM data	(2) OLS 1970-2009 Total period (balanced) (unrestricted) WDI/ PWT data	(3) OLS 1970-1988 1988-2009 Estimated separately (unbalanced) (unrestricted) WDI/ PWT data	(4) OLS 1970-1988 1988-2009 Estimated jointly (unbalanced) (unrestricted) WDI/ PWT data	(5) SUR 1970-1988 1988-2009 (unbalanced) (unrestricted) WDI/ PWT data
Intercept 1	0.235 (0.013)***	0.220 (0.000)***	0.323 (0.000)***	0.285 (0.000)***	0.339 (0.000)***
Ln wage 1970 / initial	-0.020 (0.002)***	-0.018 (0.000)***	-0.025 (0.000)***	-0.025 (0.000)***	-0.026 (0.000)***
Fertility rate 1970 / initial	-0.024 (0.002)***	-0.007 (0.000)***	-0.016 (0.000)***	-0.009 (0.000)***	-0.017 (0.000)***
Education expend 1970 / initial / H60	0.024 (0.010)*	0.000 (0.999)	-0.001 (0.345)	0.001 (0.009)**	-0.001 (0.315)
Intercept 2			0.209 (0.000)***		0.203 (0.000)***
Ln wage 1988			-0.018 (0.000)***		-0.018 (0.000)***
Fertility rate 1988			-0.005 (0.000)***		-0.005 (0.000)***
Education expend 1988			0.001 (0.071).		0.001 (0.015)*
Multiple R^2	0.68	0.67	0.54/0.33	0.44	0.53/0.33
Adjusted R^2	0.67	0.66	0.53/0.32	0.44	0.52/0.32
Num. obs.	144	139	131/515	646	131/515

Notes: The term "balanced" indicates that only cities were included, for which data was available for both periods 1970-1988 and 1988-2009, or for the total period 1970-2009. In the "unbalanced" panels additional cities, for which data was only available in one of the periods were added to benefit from the larger sample size. The term "restricted" indicates that the regressor coefficients b were restricted to be equal in both periods, while the intercepts for the two periods were allowed to vary. The standard errors are shown in brackets. In the lower section of the table where two numbers are shown separated by a slash, the first number refers to 1970-1988, while the second number refers to 1988-2009.

$^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$, $p < 0.1$

Development Indicators (WDI) dataset of the World Bank, and from the Penn World Tables 7.1 (PWT).

As proxies for population growth we initially test the fertility rate (WDI), birth rate per thousand persons (WDI), and the leading 3-year average population growth rate (PWT). For investment in human capital we initially include education expenditure as a percent of GNI (WDI) and life expectancy at birth (WDI), while the investment share of GDP (PWT) is included as a proxy for investment in physical capital. The results are similar to those when using the SDM dataset, in that again the negative coefficient on the initial wage level is highly significant in all tested specifications, but the other variables show less consistent statistical relationships, with some coefficients changing sign depending on the specification, or with coefficients exhibiting very low statistical significance. We also test the relationships across different time periods. One of the most robust relationships is shown in columns (2) to (5) of table 1.7, where the inclusion of only the fertility rate[31] and education expenditures as Solow variables results in a good model fit. This equation is

[31] A similar result is achieved when the fertility rate is replaced by the birth rate.

structurally similar to the equation in column (1) based on the SDM dataset, except that higher education (H60) has been replaced by education expenditure. Column (3) shows the results when this equation is tested separately for the sub-periods 1970-1988 and 1988-2009, while column (4) shows the results when the two sub-periods are tested simultaneously with OLS, using the values of the additional variables from their respective initial years. Column (5) provides the estimates for the coefficients estimated with a SUR model with a structure equivalent to equation (1.42), where the time periods represent the cross section. The test of simultaneous equality of the coefficients in the two periods separately on the initial wage, on the fertility rate and on education expenditures is rejected at the 0.01% level by Theil's F-Test, the Chi-squared statistic and the F-statistic of a Wald test, which points to a potential non-constant structure of conditional convergence in real wages over time.

Overall these results provide some support to the Solow-Swan model given the relatively high coefficients of determination of around 30-70% achieved with these simple regressions, as compared to 20-30% for the absolute convergence regressions. However, given the lack of evidence of any impact of investment in physical capital and the very limited evidence for the role of investment in human capital this evidence remains ambiguous. With regard to the speed of convergence, the coefficients on the initial wage point to a convergence speed σ_w of 1.0% to 1.6% per annum, somewhat faster than the unconditional rate of convergence for the full sample and equivalent time period, but slightly slower than for the conditional convergence equation when using the SDM dataset.

Next, we again test for conditional convergence in the Solow-Swan sense, but now control for fixed effects, including regions, countries, cities, and skill levels. Due to the large number of dummy coefficients that need to be estimated, we use the 1970-1988 and 1988-2009 periods jointly, with the values of the additional variables drawn from the respective initial years. We find that controlling for fixed effects does not result in the coefficients on the variables that define the steady state in the Solow-Swan model being more in-line with what the Solow-Swan theory would predict. For instance, using country fixed effects and including educational expenditure and the fertility rate as regressors (in addition to the logarithm of the initial wage) we find that the coefficient on education expenditures has a negative sign and is significant at the 0.1% level, whereas the fertility rate has a positive sign and is not statistically significant, in contrast to the theory and the results in table 1.7.[32] When additionally including life expectancy at birth and the investment share of GDP, the coefficients on all Solow control variables turn negative and are highly significant, in contrast to the theory which would predict only the fertility rate to

[32]See table B.2 in the appendix for the detailed results.

have a negative sign.[33] Replacing the country fixed effects with regional dummy variables, but now controlling also for skill levels with the lowest skill level as the baseline, we see that the coefficients on the skill dummies are positive and highly significant, pointing to stronger wage growth among more highly skilled professions (see table 1.8). In terms of regions the coefficient on the Latin America dummy is clearly most negative and highly significant, while South-East Asia dummy is also negative and significant at the 0.1% level. However, again the signs of two of the coefficients on the variables that control for the steady state in the Solow-Swan model are not in line with the theory; the investment share of GDP and education expenditures have negative signs and very small coefficient estimates (and are not statistically significant). In contrast, life expectancy at birth and the fertility rate are significant and in line with conditional convergence in the Solow-Swan model. Interestingly, controlling for skills and regions in this way results in a high convergence coefficient, pointing to a convergence speed σ_w of 2.1%.

Table 1.8: Wage regression estimates jointly for the periods 1970-1988 and 1988-2009 on profession-level data, including the fertility rate, education expenditure, life expectancy at birth and investment share of GDP as steady state control variables, and skill and region fixed effects (WDI / PWT data)

	Solow-Swan conditional convergence with four steady state control variables and skill and region fixed effects
Intercept	0.290 (0.022)***
Ln initial wage	−0.031 (0.001)***
Education expenditure	−0.000 (0.001)
Fertility rate	−0.007 (0.001)***
Life expectancy at birth	0.001 (0.000)*
Investment share of GDP	−0.000 (0.000)
factor(South-East Asia)1	−0.009 (0.003)**
factor(Europe)1	0.002 (0.003)
factor(Latin America)1	−0.018 (0.003)***
factor(Skills)Level2	0.013 (0.002)***
factor(Skills)Level3	0.022 (0.002)***
Multiple R^2	0.564
Adjusted R^2	0.557
Num. obs.	646

Notes: The standard errors are shown in brackets.

$^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$, $p < 0.1$

[33]See table B.3 in the appendix for the detailed results.

Winners and Losers of Convergence The positive coefficient estimates for the skill level dummies when unskilled professions (skill level 1) represent the baseline as in table 1.8, point to higher average wage growth for medium skilled and even higher wage growth for highly skilled professions. This provides some evidence as to the winners and losers of convergence, in response to Williamson's (1996, p. 300) questions on "who gains and who loses from convergence".

Williamson (1996, p. 300) asks, "(w)hat about wages of unskilled laborers, wages of skilled artisans, salaries of skilled clerical workers, farm rents accruing to landlords, and profits accruing to capitalists?" For more granular evidence on the winners and losers of convergence, we replace the skill dummies by profession dummies, comparing whether individual professions have experienced different average growth rates, with car mechanics representing the baseline. We show the results for 1988-2009 as in this later period a larger number of data points is available for each profession, see table 1.9. While average growth in wages was practically identical for car mechanics and bus drivers, it was lower for saleswomen and lowest for construction workers and female factory / textile workers. It was clearly highest for department managers and electrical engineers (the two highest skilled professions in our sample), with the group showing the next highest wage increases consisting of bank tellers / credit clerks, cooks and skilled industrial workers. Primary school teachers and secretaries experienced average wage increases that were somewhat higher than the baseline. Overall these results also support the thesis, that the highest skilled professions around the world experienced the highest wage growth in this period, and that low skilled workers experienced the lowest wage growth. Eight out of eleven of the non-baseline professions had coefficients that were statistically significant at least at the 5% level.[34]

This finding can be interpreted as evidence for skill-biased technical change that has resulted in a higher demand for skilled labour relative to unskilled labour, thereby increasing wages rates for the skilled faster, than for the unskilled.

Conditional Convergence over a 3-year Horizon So far, the focus has been on longer term wage growth. We now turn to the question, to what extent shorter term changes in wages can be explained within the context of conditional convergence in the Solow-Swan framework. For this we regress the 3-year wage growth rates across all survey years jointly on the logarithm of initial wages, as well as the additional variables that control for the steady state, drawn from the respective initial years.[35] The results for five

[34]The panel data from the World Bank WDI and PWT 7.1 datasets is employed in this regression. Very similar results are achieved when using the SDM dataset.

[35]The panel data from the World Bank WDI and PWT 7.1 datasets is again employed.

Table 1.9: Wage regression estimates for 1988-2009 on profession-level data, including the fertility rate, education expenditure, life expectancy at birth and investment share of GDP as steady state control variables, and professions (car mechanics = baseline) and region fixed effects (WDI / PWT data)

	Solow-Swan conditional convergence with four steady state control variables and profession and region fixed effects
Intercept	0.306 (0.022)***
Ln initial wage	−0.031 (0.001)***
Education expenditure	0.001 (0.001)
Fertility rate	−0.010 (0.002)***
Life expectancy at birth	0.001 (0.000)**
Investment share of GDP	−0.000 (0.000)*
factor(South-East Asia)1	−0.012 (0.003)***
factor(Europe)1	−0.010 (0.003)**
factor(Latin America)1	−0.018 (0.003)***
factor(Profession)Bank.Tellers.or.Credit.Clerks	0.018 (0.003)***
factor(Profession)Bus.Drivers	0.001 (0.003)
factor(Profession)Construction.Workers	−0.007 (0.003)*
factor(Profession)Cooks	0.018 (0.003)***
factor(Profession)Department.Managers	0.034 (0.004)***
factor(Profession)Electrical.Engineers	0.022 (0.003)***
factor(Profession)Factory.or.Textile.Workers	−0.007 (0.003)*
factor(Profession)Industrial.Workers	0.016 (0.003)***
factor(Profession)Primary.School.Teachers	0.010 (0.003)**
factor(Profession)Saleswomen	−0.004 (0.003)
factor(Profession)Secretaries	0.003 (0.003)
Multiple R^2	0.559
Adjusted R^2	0.542
Num. obs.	514

Notes: The standard errors are shown in brackets.

$^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$, $p < 0.1$

different specifications are shown in table 1.10.

The most noticeable change as compared to the analyses when using wage growth data over the complete 1970-2009 period or the sub-periods 1970-1988 and 1988-2009 is that the convergence coefficient on the logarithm of the initial wage for the 3-year periods is systematically higher, resulting also in a higher speed of convergence σ_w of between 2.0% to 4.6% for the specifications in table 1.10. The impact of life expectancy at birth and the investment share of GDP is very small (see columns (2) to (5)), and the impact of education expenditure is not much higher. The fertility rate again shows the clearest impact on wage growth and is highly statistically significant. In the SUR regressions, using the periods as the cross section in column (3) and the skill levels as the cross section in column (4), the signs of the coefficients are mostly in line with the Solow-Swan conditional convergence theory, but the statistical significance of the coefficients is limited for the

Table 1.10: Profession-level 3-year wage growth regressed on initial profession-level wages and additional variables that define the Solow-Swan steady state (conditional convergence) between 1970 and 2009

	(1) OLS	(2) OLS	(3) SUR Periods 1: 1970-1988 2: 1988-2009	(4) SUR Skill levels 1, 2 and 3	(5) OLS + Fixed effects (regions and skills)
	3-year growth rates 1970-2009 Estimated jointly (unbalanced)	3-year growth rates 1970-2009 Estimated jointly (unbalanced)	3-year growth rates (unbalanced)	3-year growth rates 1970-2009 (unbalanced)	3-year growth rates 1970-2009 Estimated jointly (unbalanced)
Intercept 1	0.573 (0.000)***	0.574 (0.000)***	0.996 (0.000)***	0.809 (0.002)**	0.630 (0.000)***
Ln initial wage 1	-0.052 (0.000)***	-0.051 (0.000)***	-0.043 (0.000)***	-0.090 (0.002)**	-0.067 (0.000)***
Education expend initial 1	0.003 (0.001)***	0.004 (0.000)***	0.013 (0.000)***	0.020 (0.099).	0.002 (0.086).
Fertility rate initial 1	-0.024 (0.000)***	-0.025 (0.000)***	-0.061 (0.000)***	-0.046 (0.022)*	-0.021 (0.000)***
Life expectancy at birth initial 1		-0.000 (0.486)	-0.006 (0.000)***	0.001 (0.815)	0.001 (0.076).
Investment share of GDP initial 1		0.000 (0.024)*	-0.000 (0.528)	0.001 (0.701)	0.001 (0.007)**
Intercept 2			0.491 (0.000)***	0.699 (0.010)**	
Ln initial wage 2			-0.049 (0.000)***	-0.050 (0.020)*	
Education expend initial 2			0.006 (0.007)**	0.013 (0.211)	
Fertility rate initial 2			-0.027 (0.000)***	-0.034 (0.069).	
Life expectancy at birth initial 2			0.000 (0.951)	-0.002 (0.395)	
Investment share of GDP initial 2			0.001 (0.003)**	-0.000 (0.834)	
Intercept 3				0.522 (0.024)*	
Ln initial wage 3				-0.061 (0.002)**	
Education expend initial 3				0.000 (0.960)	
Fertility rate initial 3				-0.039 (0.025)*	
Life expectancy at birth initial 3				0.002 (0.504)	
Investment share of GDP initial 3				0.003 (0.113)	
factor(South-East Asia)1					-0.014 (0.009)**
factor(Europe)1					0.012 (0.007)**
factor(Latin America)1					-0.019 (0.000)***
factor(Skills)Level2					0.026 (0.000)***
factor(Skills)Level3					0.054 (0.000)***
Multiple R^2	0.12	0.12	0.12/0.14	0.13/ 0.05/ 0.22	0.15
Adjusted R^2	0.12	0.12	0.11/0.13	0.06/ -0.02 /0.16	0.15
Num. obs.	6588	6588	528/990	74/74/74	6286

Notes: The term "unbalanced" indicates that all cities were included for which the respective 3-year growth rates are available. This also means that for different 3-year intervals different numbers of cities were included. The standard errors are shown in brackets. In the lower section of the table where numbers are shown separated by a slash, the numbers refer to the two sub-periods 1970-1988 and 1988-2009 in column (3), and to the skill levels 1, 2 and 3 in column (4).

$^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$, $p < 0.1$

regressions across the skill levels. When including fixed effects as shown in column (5), the results from table 1.8 are confirmed: Wage growth was all the stronger the higher the skill levels of the professions, and Latin America as a region and to a lesser extent South-East Asia had slower wage growth.

1.7.4 Conditional Convergence in Profession-Level Wages with Additional Explanatory Variables

We now include a vector z of additional variables: In the context of the Solow model they might be considered as additional controls for the steady state. In the context of endogenous growth models these variables would usually be considered as stand-alone drivers of economic growth. The statistical rational for adding these variables is to avoid omitted variable bias, which could result in the estimated coefficients and the explanatory power appearing to be larger than they are. The focus of this section is wider than that of the previous sections that aimed mainly at testing for absolute or conditional convergence. Rather, this section also aims to investigate the determinants of wage growth more broadly.

Clearly, the procedure selected for assessing the effect of a range of different factors on wage growth will have a substantial impact on the results, with different procedures resulting in potentially very different results. As mentioned previously, a variety of techniques have been employed in an attempt to improve identification in regressions of GDP per capita, including Bayesian, pseudo-Bayesian and frequentist model averaging estimators, general-to-specific modelling, principal components augmented regressions and adaptive lasso sequences. However, none has established itself as a benchmark and results differ quite substantially between different approaches, pointing to the underlying difficulty of theory open-endedness. We choose to use principal components augmented regressions (PCAR, cf. Hlouskova and Wagner, 2010) due to this procedure's efficiency in tackling two important problems: First, the uncertainty about the relevance of the variables is addressed by calculating conditional *individual* effects. Second, degrees of freedom can be used very efficiently by including only those principal components that capture the lion's share of all factors' variance (cf. Hlouskova and Wagner, 2010).

As dependent variables we use profession-level wage growth rates over the total 1970-2009 period (see table 1.11), and separately, over 3-year intervals (see table 1.12). The independent variables included had to be available for a wide range of countries since 1970 in three year intervals, or they had to be largely time-independent and thus assumed to be constant over the 1970-2009 period. As independent variables we include from the SDM dataset dummy variables for former colonies, former British and Spanish colonies, and regional dummies for South-East Asia, Europe, Latin-America and Sub-Saharan Africa; socio-economic variables including an index of ethnolinguistic fractionalisation, an index of religious intensity, the fraction of a country's population living in the geographical tropics, and the fraction speaking English; geographical information such as whether a country is landlocked, the absolute latitude, the distance to the closest of the trading hubs Rotterdam,

Table 1.11: Principal components augmented regressions: Profession-level wage growth for 1970-2009 regressed on each independent variable while controlling for the principal components (PCs) with eigenvalues larger than one derived from all other independent variables included in the model

	(1) OLS with initial wage regressor but without PCs 1970-2009	(2) OLS with initial wage included in PCs 1970-2009	(3) OLS with initial wage regressor and PCs 1970-2009
Ln initial wage	-0.013 (0.002)***	NA	-0.020 (0.003)***
Birth rate per thousand population	-0.001 (0.000)***	-0.002 (0.000)***	-0.001 (0.000)***
Fertility rate	-0.007 (0.000)***	-0.016 (0.002)***	-0.008 (0.002)***
Life expectancy at birth	0.001 (0.000)***	0.001 (0.000)**	0.000 (0.000)
Old age dependency ratio	0.001 (0.000)***	0.001 (0.000)*	0.000 (0.000)
Youth age dependency ratio	-0.000 (0.000)***	-0.001 (0.000)***	-0.001 (0.000)***
Education expenditure in % of GNI	0.001 (0.001)**	-0.002 (0.001)*	-0.002 (0.001)*
Investment as a share of GDP	0.000 (0.000)	-0.000 (0.000)	-0.000 (0.000)*
Consumption as a share of GDP	-0.001 (0.000)***	0.002 (0.000)***	-0.000 (0.000)
Government consumption as a share of GDP	0.001 (0.000)*	-0.002 (0.001)*	-0.000 (0.000)
Openness	0.000 (0.000)***	-0.000 (0.000)	0.000 (0.000)
Trade in percent of GDP	0.000 (0.000)***	-0.000 (0.000)	0.000 (0.000)
Size of PPP GDP	0.000 (0.000)*	0.000 (0.000)	0.001 (0.000)
Consumer price inflation, annual in %	-0.000 (0.000)***	-0.000 (0.000)	-0.000 (0.000)
Absolute latitude	0.000 (0.000)***	0.000 (0.000)*	-0.000 (0.000)
Distance from New York, Rotterdam or Tokyo	-0.000 (0.000)***	0.000 (0.000)	0.000 (0.000)*
Land area in km2	-0.000 (0.000)*	-0.000 (0.000)*	-0.000 (0.000)
Fraction of land near navigable water	0.012 (0.003)***	-0.016 (0.004)***	0.000 (0.003)
Fraction of population living in tropics	-0.041 (0.005)***	0.012 (0.012)	0.022 (0.009)*
Fraction of country in tropical climate zone	-0.045 (0.007)***	0.011 (0.016)	0.049 (0.011)***
Index of ethnolinguistic fractionalization	-0.003 (0.006)	0.006 (0.014)	-0.016 (0.009).
Fraction of the population speaking English	0.009 (0.003)**	0.020 (0.004)***	0.008 (0.004)*
Religious intensity	-0.010 (0.005)*	0.001 (0.008)	-0.011 (0.006).
Skill level 2 (dummy)	0.002 (0.002)	0.002 (0.002)	0.003 (0.001)*
Skill level 3 (dummy)	0.005 (0.003)	0.005 (0.002)*	0.006 (0.002)**
Emerging market (dummy)	-0.018 (0.002)***	0.016 (0.007)*	-0.003 (0.006)
Former colony (dummy)	-0.009 (0.002)***	0.001 (0.005)	-0.004 (0.004)
Former British colony (dummy)	0.004 (0.002).	0.002 (0.004)	-0.001 (0.003)
Former Spanish colony (dummy)	-0.018 (0.003)***	-0.008 (0.007)	0.005 (0.005)
South-East Asia (dummy)	0.012 (0.004)**	-0.008 (0.004)*	0.009 (0.003)**
Europe (dummy)	0.015 (0.002)***	-0.008 (0.005).	-0.003 (0.004)
Latin-America (dummy)	-0.022 (0.002)***	-0.045 (0.021)*	-0.070 (0.016)***
Sub-Saharan Africa (dummy)	-0.014 (0.005)**	0.023 (0.007)**	-0.006 (0.005)
Landlocked (dummy)	0.008 (0.003)**	0.004 (0.003)	0.004 (0.002)

***$p < 0.001$, **$p < 0.01$, *$p < 0.05$, $p < 0.1$

New York or Tokyo,[36] the fraction of a country's surface area within 100km of navigable

[36]This distance was missing for Luxembourg in the SDM dataset and was added manually.

water,[37] the fraction of the surface area in a tropical climate zone, and the land area in square kilometres. From the WDI dataset we include the birth rate (per thousand of the population), the fertility rate, life expectancy at birth, the old/youth age dependency ratios, education expenditure in percent of GNI,[38] consumer price inflation,[39] and trade in percent of GDP.[40] From the PWT 7.1 dataset we include the consumption and investment shares of GDP, government consumption as a share of GDP, the size of PPP-adjusted GDP, and a measure for economic openness reflecting the extent of external trade relations. Finally, we include dummy variables for skill levels and for emerging markets. With this choice of independent variables and after some additional data points were added from other sources (see the relevant footnotes) we can include all 144 wage growth data points used previously for the total period 1979-2009. However, of the 7001 profession-level 3-year growth rates available from the data, the panel of additional variables is complete only for 5728 cases. This is related to the fact that in later years of the UBS Prices and Earnings survey many new cities for which good regressor data is not available were added to the survey, e.g. cities in Central and Eastern Europe, and in CIS countries.

We follow the methodology of Hlouskova and Wagner (2010) and calculate individual conditional effects by regressing wage growth on each independent variable separately, while controlling for the principal components (with eigenvalues larger than one) derived from all other independent variables included in the model. As proposed by Hlouskova and Wagner (2010), we determine principal components separately for the quantitative and dummy variables to account for their differing characteristics while determining the principal components. In all cases the principal components are computed using the correlation matrix, which takes account of the different scalings of the quantitative variables. In table 1.11 for the total period growth rates and table 1.12 for the 3-year growth rates column (1) shows the coefficient results of the simple OLS regression of the wage growth variable on each individual regressor, while controlling only for the logarithm of the initial wage. In column (2) we control for the principal components with eigenvalues larger than one calculated from all regressors (including the logarithm of the initial wage). In column (3) the logarithm of the initial wage is taken as a stand-alone regressor, while the principal

[37] The fraction of a country's surface area within 100km of navigable water was missing from the SDM dataset for Luxembourg and Manama and were added manually.

[38] Education expenditure for 1970 in South Africa was missing from the WDI dataset. We approximate this number using the education expenditure figure for 1960-1965 from the SDM database.

[39] Consumer price inflation data was missing in the WDI dataset for some cities including Hong Kong, London, and Brazil for some years. For these countries a number of inflation data points could be added using information from the countries' central banks and statistical offices.

[40] Trade as a percent of GDP for Switzerland, which is missing in the original SDM dataset, was calculated for individual years from the tables Q.2 and L.3 of the Research Center for Social and Economic History of the University of Zurich, provided at http://www.fsw.uzh.ch/hstat/nls/overview.php.

components are calculated from all other regressors.

Table 1.12: Principal components augmented regressions: Profession-level wage growth for 3-year intervals between 1970 and 2009 regressed on each independent variable while controlling for the principal components (PCs) with eigenvalues larger than one derived from all other independent variables included in the model

	(1) OLS with initial wage regressor but without PCs 1970-2009	(2) OLS with initial wage included in PCs 1970-2009	(3) OLS with initial wage regressor and PCs 1970-2009
Ln initial wage	-0.031 (0.002)***	NA	-0.062 (0.003)***
Birth rate per thousand population	-0.003 (0.000)***	-0.005 (0.001)***	-0.000 (0.000).
Fertility rate	-0.024 (0.002)***	-0.040 (0.004)***	-0.006 (0.003).
Life expectancy at birth	0.003 (0.000)***	0.000 (0.001)	-0.001 (0.001)
Old age dependency ratio	0.003 (0.000)***	0.000 (0.001)	-0.001 (0.001)
Youth age dependency ratio	-0.002 (0.000)***	-0.002 (0.000)***	-0.000 (0.000)
Education expenditure in % of GNI	0.003 (0.001)***	0.005 (0.002)**	-0.001 (0.002)
Investment as a share of GDP	0.001 (0.000)***	-0.000 (0.000)	-0.000 (0.000)
Consumption as a share of GDP	-0.001 (0.000)***	-0.001 (0.000)***	-0.000 (0.000)
Government consumption as a share of GDP	0.000 (0.000)	-0.007 (0.001)***	-0.001 (0.001)
Openness	0.000 (0.000)	-0.000 (0.000)*	-0.000 (0.000)
Trade in percent of GDP	0.000 (0.000)	0.000 (0.000)***	0.000 (0.000)
Size of PPP GDP	0.000 (0.000)***	-0.000 (0.000)	0.000 (0.000)
Consumer price inflation, annual in %	0.000 (0.000)	0.000 (0.000)*	0.000 (0.000)**
Absolute latitude	0.000 (0.000)***	0.001 (0.000)**	0.001 (0.000)**
Distance from New York, Rotterdam or Tokyo	-0.000 (0.000)***	-0.000 (0.000)***	-0.000 (0.000)***
Land area in km2	0.000 (0.000)*	0.000 (0.000).	-0.000 (0.000)
Fraction of land near navigable water	0.008 (0.004)*	0.010 (0.006)	0.014 (0.006)*
Fraction of population living in tropics	-0.066 (0.007)***	0.019 (0.011).	0.011 (0.009)
Fraction of country in tropical climate zone	-0.054 (0.006)***	0.030 (0.015)*	0.025 (0.014).
Index of ethnolinguistic fractionalization	-0.038 (0.006)***	0.024 (0.011)*	0.023 (0.011)*
Fraction of the population speaking English	0.025 (0.004)***	0.054 (0.008)***	0.026 (0.008)**
Religious intensity	-0.005 (0.006)	-0.016 (0.012)	0.016 (0.012)
Skill level 2 (dummy)	0.012 (0.003)***	0.001 (0.003)	0.027 (0.003)***
Skill level 3 (dummy)	0.026 (0.004)***	0.002 (0.003)	0.059 (0.004)***
Emerging market (dummy)	-0.037 (0.003)***	-0.018 (0.006)**	0.004 (0.006)
Former colony (dummy)	-0.020 (0.003)***	-0.005 (0.005)	-0.028 (0.005)***
Former British colony (dummy)	0.001 (0.003)	0.020 (0.005)***	-0.014 (0.005)**
Former Spanish colony (dummy)	-0.026 (0.004)***	0.051 (0.008)***	0.029 (0.008)***
South-East Asia (dummy)	-0.006 (0.003).	-0.004 (0.006)	-0.014 (0.005)**
Europe (dummy)	0.033 (0.003)***	0.021 (0.007)**	-0.019 (0.006)**
Latin-America (dummy)	-0.032 (0.004)***	-0.056 (0.011)***	-0.033 (0.011)**
Sub-Saharan Africa (dummy)	-0.033 (0.007)***	0.011 (0.009)	0.042 (0.009)***
Landlocked (dummy)	0.012 (0.004)**	0.003 (0.005)	0.008 (0.004).

***$p < 0.001$, **$p < 0.01$, *$p < 0.05$, $p < 0.1$

Regarding the independent variables that were included in the Solow-type OLS, SUR and fixed effect regressions in the previous sections, the principal components augmented regression results in tables 1.11 and 1.12 confirm the statistical significance, consistency of the signs of the coefficient estimates and the large size of the effect of the logarithm of the initial wage (negative sign), the fertility rate (negative sign), and the Latin-America dummy (negative sign). The coefficients on the skill level dummy variables are consistently positive and also statistically significant, but the size of the effects is smaller. Of the new additional variables one stands out as having coefficients that are statistically significant, large in size and that have consistent signs: The coefficient on the fraction of the population speaking English points to a large positive and statistically significant impact on wage growth. Once principal components are accounted for, the sign on coefficient estimates of the fraction of the population living in the tropics and the fraction of land in tropical climate zones turns positive and the coefficients become quite large, whereas they were negative and large when including only the initial wage and these specific regressors (column (1)). For more than half of the regressors, however, the effects are small.

The most noticeable difference in the analysis over the total period 1970-2009 versus the analysis over the 3-year periods is again the markedly larger absolute size of the coefficient on the initial wage. For the 3-year growth rates the coefficient on the initial wage implies a speed of convergence σ_w of close to 3% per annum, whereas for the total period it is 1.7%. Overall this analysis provides a strong alternative confirmation of conditional convergence of profession-level wages, both over the 3-year periods and over the total 1970-2009 period.

1.8 Summary and Conclusion

In this chapter we have analysed the convergence hypothesis for purchasing power adjusted wages across the world using wage data from the UBS Prices and Earnings surveys for 1970 to 2009. This data is advantageous as compared to other data sources, due to its consistent definition of profession-level wages across cities, and over time, allowing for more detailed analyses (in particular with regard to skill levels) than in past studies. We derive the theoretical basis for wage convergence within the Solow-Swan model of economic growth, but show that wage convergence can be viewed also within the contexts of trade theory, and migration theory.

Absolute convergence We find no clear statistical evidence of an absolute catch-up in wages for poorer countries as measured by *GDP per capita*, but we find solid evidence of absolute convergence in two senses: Cities with lower *average* initial real *wages* exhibit higher growth in *average wages*; and lower initial *profession-level* real *wages* world wide (whether due to the country context or the profession) also exhibit higher growth. Convergence in these senses is stronger in the latter half of our observation period 1988-2009, but absolute convergence holds also for the complete period 1970-2009. Evidence of absolute convergence is even stronger when the sample is restricted to more homogenous groups of countries by subdividing the sample into developed and emerging markets. Convergence is also stronger within more homogenous skill groups: It is strongest within individual professions across the world, somewhat less strong within professions clustered according to their skill levels, and least strong when all professions are included.

Conditional convergence We test for conditional convergence in the Solow-Swan sense by including variables that control for the steady state in the extended Solow-Swan model. Not all of the variables included are statistically significant, but using the initial wage level, the fertility rate and an education measure as regressors provides a good model fit. The results provide some support to the Solow-Swan model given the relatively high coefficients of determination of around 30-70% achieved with these simple regressions, as compared to 20-30% for the absolute convergence regressions. However, given the lack of evidence of any impact of investment in physical capital and the very limited evidence for the role of investment in human capital this evidence remains ambiguous. With regard to the speed of convergence, the coefficients on the initial wage point to a convergence speed σ_w of 1.0% to 1.7% per annum, somewhat faster than the unconditional rate of convergence for the full sample and equivalent time period of about 1.0% per annum.

Winners and losers We aim to provide more granularity on the question of conditional convergence, by investigating who the winners and losers of the convergence trends have been. Compared to low-skilled professions, we find higher average wage growth for medium-skilled, and even higher wage growth for highly-skilled professions. This finding can be interpreted as evidence for skill-biased technological change that has resulted in a higher demand for skilled labour relative to unskilled labour, thereby increasing wage rates for the skilled faster, than for the unskilled. This result does not contradict the finding of convergence described above, but rather gives insight into how convergence came about. Wage convergence across the world was *not* based on lower skilled professions gaining on higher skilled professions. Rather, the primary driver of international wage convergence was faster overall growing wage levels in lower wage countries compared to higher wage countries.

Wage growth determinants Finally, we use principal components augmented regressions to analyse the impact of a broader range of variables on real wage growth. The initial wage and the fertility rate are confirmed as consistently having a large and statistically significant impact on wage growth. Cities in Latin America experienced statistically significant lower levels of wage growth and skill level dummy variables point to higher wage growth in professions with higher skill levels. Additionally, the fraction of the population speaking English was found to have a large positive and statistically significant impact on wage growth.

Conclusion Overall these results support the hypotheses of absolute and conditional convergence in real wages, with faster overall growing wage levels in lower wage countries, as compared to higher wage countries being the key driver. At the same time, the highest skilled professions around the world have experienced the highest wage growth during 1970-2009, while low skilled workers experienced the lowest wage growth, thus no convergence in this sense is found between skill groups. This more differentiated understanding of convergence trends in real wages provides important insights into the potential for the side-by-side existence of convergence and rising regional wage inequality, in a world in which the latter is increasingly acting as a driver for social discord and unrest.

Chapter 2

Factor Price Equalisation and Relative Wages: An Empirical Investigation

2.1 Introduction

In the first chapter, convergence in global wages – in the absolute and in the conditional sense of Solow-Swan – is a strong and robust result. However, in spite of this convergence, large disparities in international wages persist. This conflicts with the theory of Factor Price Equalisation (FPE), which goes beyond convergence, asserting that under certain conditions *trade in goods can act as a substitute for factor mobility*, implying that *wages will equalise across different regions even without factor flows* between countries.[1] The theory of Factor Price Equalisation (FPE) can therefore be viewed as the production side analog to Purchasing Power Parity (PPP) on the consumption side. However, while PPP relies on consumers engaging in trade until no more arbitrage opportunities exist, FPE must manage without arbitrage functioning between its factor markets, as this would require capital and labour to move freely between countries. While globalisation has accelerated both migration and FDI flows, hurdles, in particular to the mobility of labour, remain high.

This chapter contributes to empirically explaining the origin of deviations from FPE by drawing a parallel to the analogous "price" problem – i.e. the origin of deviations from PPP. As PPP would imply a constant real exchange rate of one, fluctuations in the real exchange rate reflect deviations from PPP theory. In 1993 Charles Engel set an important

[1] See Heckscher (1919) and Ohlin (1933).

empirical yardstick for models of the real exchange rate by decomposing the expression for the real exchange rate and analyzing the variability of the parts (i.e. variability of relative consumer prices within and between countries). By identifying the origin of volatility in the real exchange rate, Engel (1993) pinpoints the origin of deviations from PPP. His descriptive analysis thus provides a guideline for theories attempting to explain deviations from PPP – they should take account of where empiric volatility comes from in the real exchange rate.

We investigate the equivalent relationship between the real exchange rate and relative wages by decomposing an adapted expression for the real exchange rate, now based on factor prices (wages), instead of goods prices. By replacing goods prices by wages, we in essence create a real exchange rate for the production side of the economy. FPE would imply that this new measure of the real exchange rate based on wages should equal one. Thus, analogously to Engel's (1993) approach we investigate whether movements in the real exchange rate based on wages have historically been better explained by the variability in the wages between countries, or by the variability of wages within countries.

While the decomposition of the real exchange rate based on prices that Engel uses has been widely discussed in the literature for many years, the relationship between wages and the real exchange rate has come into the spotlight only more recently. For example, Jinjarak and Naknoi (2010) propose a model in which the real exchange rate is driven by the variation in relative wages and the seller's markup. Mishra and Spilimbergo (2009) calculate the elasticity of wages and the real exchange rate for different levels of integration between labor markets. Bacsafra (2005) investigates whether departures from PPP can be attributed to wages and Nucci and Pozzolo (2009) find a statistically significant relationship between currency variations and wages in Italy. This chapter aims to contribute to further these debates by pinpointing the origin of deviations from FPE. As in Engel's case for PPP, this provides an empirical yardstick for theories explaining divergences from FPE.

The structure of this chapter is as follows: Section 2.2 provides an overview of FPE theory. The approach selected in this chapter to explain deviations from FPE is motivated in section 2.3 by drawing the analogy to Engel's (1993) work. Section 2.4 describes the data employed, while section 2.5 expounds the chosen methodology. In section 2.6 the results for the selected wage subindices and profession-level wages are presented, while section 2.7 summarises the conclusions.

2.2 Factor Price Equalisation: Theory and Evidence

Classical trade theory argues that in efficient markets where there are no barriers to trade or transportation costs, the prices of traded goods in different countries will equalise. This is often referred to as the *law of one price* for identical goods, or *purchasing power parity (PPP)* when referring to a broader basket of goods. The same is inferred for productive factors, i.e. if capital and labour can move freely between countries their relative and absolute prices (rents, wages) should equalise. However, perfect mobility of capital and labour is a far more awkward assumption than free trade – in particular, hurdles to the mobility of labour remain high.

The Heckscher-Ohlin trade model finds that differences in factor endowments combined with limits to the mobility of production factors can, in fact, be a driver for trade. Further, under certain conditions trade in goods can act as a substitute for factor mobility, implying that factor prices will converge even without factor mobility between countries. This occurs because as countries engage in trade they increase exports of goods that intensively use the factors with which they are highly endowed, and import more goods intensive in factors with which they are only poorly endowed. As algebraically described by Stolper and Samuelson (1941), in each country this increases the relative demand for the more abundant (and cheaper) factor, while demand for the scarcer (and more expensive) factor falls, putting pressure on its price. In this way trade in goods can induce via a shift in factor demand an adjustment of factor prices. Further, under specific conditions an invertible mapping exists between the vector of goods prices and the vector of factor prices, through which goods prices uniquely set factor prices. While this result, termed the Factor Price Equalisation (FPE) theorem (see Samuelson, 1948, and Lerner, 1952), has become a key component in international trade theory and reflects intuitive expectations of how trade could affect wages, tremendous divergences in international wages persist.

From a theoretical perspective the restrictive conditions of the original FPE theorem[2] within the context of the 2x2x2 Heckscher-Ohlin model are an important first constraint that might explain these divergences. However, extensions of Heckscher-Ohlin to more

[2]Conditions include no barriers to trade, no transportation costs, perfect competition and full employment, factors mobile within a country but immobile across national borders, no complete specialisation, production functions exhibit constant returns to scale and differ among industries, identical technology between trading countries, and no factor intensity reversal. The condition that factor endowments should not be too different, i.e. in the same "cone (or lens) of diversification" was later extended to a "multi-cone" version in which FPE occurs among countries within the same cone, see, for instance, Deardorff (2001). As Rassekh and Thompson (1993) note: "The critical condition for FPE is deceptively simple: the number of factors must not be greater than the number of international markets (exogenous prices) (...). If the number of factors is greater than the number of exogenous prices, FPE does not follow since different vectors w of factor prices support the competitive equilibrium."

generalised contexts including more goods, factors, non-traded goods and market imperfections[3] and in particular specifications of general equilibrium models *where FPE does not necessarily hold* suggest that FPE *will nearly hold* between trading partners.[4] This Near FPE applies across a wide range of production functions (see Thompson, 1990, and Thompson, 1997).

Similar to what is frequently suggested for PPP, Hicks (1959, p. 267) suggests that FPE might be viewed as a longer term tendency: Just as PPP does not systematically hold in the short term, it is nonetheless considered a valuable guide in the longer run. Thus, while FPE is not expected to hold in the short term in its original absolute formulation Samuelson (1971) rightly directed the discussion towards Factor Price Convergence (FPC): If factor prices converge over time as trade barriers and transportation costs fall, then the mechanisms at the core of FPE theory could be at work.[5]

Evidence from economic historians suggests that in periods during which competing goods were traded,[6] trade barriers were reduced and trade volumes expanded, factor prices tended to converge. In fact, O'Rourke et al. (1996) find substantial support for FPC in the late nineteenth century, a period of strong globalisation, which they call a "dramatic historical episode of factor price convergence". It was with this background that Eli Heckscher in 1919 formulated the Heckscher-Ohlin framework of international trade. In Heckscher and Ohlin's words, "trade increase[d] the price of land in Australia and lower[ed] it in Europe, while tending to keep wages down in Australia and up in Europe" (Heckscher, 1919, p. 91f). During parts of the first half of the 20th century trade and globalisation went into reverse, due to the wars, but also due to the fateful economic nationalism of the 1930s. While the second half of the 20th century was again characterized by increased globalisation and falling trade barriers in the context of the General Agreement on Tariffs and Trade (GATT) and the WTO, the empirical evidence for FPE during this period remains somewhat ambiguous.[7] While evidence for FPC or FPE is found by, among others, Tovias (1982), Gremmen (1985), Dollar and Wolff (1988), Mokhtari and Rassekh (1989),

[3]See among others Ethier (1974), Chang (1979), Takayama (1982), and Thompson (1987).

[4]Rassekh and Thompson (1993, p. 6).

[5]The Specific Factors model with homothetic demand (see, for instance, Samuelson, 1971) in which FPC occurs with free trade is frequently considered the shorter term version of FPE in the Heckscher-Ohlin context. See also Thompson (1994) for an alternate formulation of the Specific Factors model; here Near FPE is a robust result.

[6]O'Rourke and Williamson (1999) find that before the 19th century mostly goods exclusive to certain regions, i.e. non-competing goods such as spices, silk, sugar, gold and silver, were traded. Clearly, this type of trade did not induce goods price equalisation nor FPE.

[7]O'Rourke and Williamson (1999) suggest that the more rapid technological changes and higher skill differentiation between countries over the past decades might have weakened the power of the Heckscher-Ohlin model.

O'Rourke and Williamson (1992), Rassekh and Thompson (1993) and Madsen (1996), additionally to the evidence for convergence in wages presented in the first chapter, a comparable list of studies can be cited that find that FPE does not hold, with Davis and Mishra (2007) among the more recent critics. While FPE theory remains controversial and empirical evidence is ambiguous, it continues to be recognized as an important long-run framework within international trade theory. As such, we aim to identify the origin of deviations from FPE by drawing a parallel to the analogous price problem in the next section.

2.3 FPE Analogy to PPP, and Engel's 1993 Model

Engel (1993) published what he called a "striking empirical regularity" (Engel, 1993, p. 35): his analysis found a far higher variability of consumer prices of the same goods in different countries, than the variability between different goods within a country. This was important as it provided an empirical yardstick for models explaining the deviations from constant real exchange rates that would be expected under purchasing power parity. As recalled by Engel (1993, p. 36f) most models of the real exchange rate rely on representations for the real exchange rate similar in structure to the two examples below. Let the CPI-based real exchange rate be defined as

$$RER^{CPI} = \frac{P}{S \cdot P^*} \tag{2.1}$$

with P and P^* the Consumer Price Index in the home and foreign country and S the exchange rate (units of local currency per unit of foreign currency). As a first example with two goods which are consumed and traded in both countries, but have different weights a and b in the respective CPI's, the real exchange rate can be written as (with lower case letters referring to natural logs)

$$p - s - p^* = a(p_1 - s - p_1^*) + (1-a)(p_2 - s - p_2^*) + (b-a)(p_2^* - p_1^*) \tag{2.2}$$

with

$$p = a \cdot p_1 + (1-a)p_2 \quad and \quad p^* = b \cdot {p_1}^* + (1-b)p_2^*. \tag{2.3}$$

Models explaining the variability of the real exchange rate based on the price differences of comparable goods in different countries would expect to see significant variability in the first two terms on the right hand side of equation (2.2). In contrast, models based on the variability of prices of different goods in individual countries would expect more variability in the last term on the right hand side of equation (2.2).

Similarly, in the second example, with two countries both consuming a traded and a non-traded good, the real exchange rate is

$$p - s - p^* = (p_T - s - p_T^*) + (1-a)(p_N - p_T) - (1-b)(p_N^* - p_T^*) \tag{2.4}$$

with

$$p = a \cdot p_T + (1-a)p_N \quad and \quad p^* = b \cdot p_T^* + (1-b)p_N^*. \tag{2.5}$$

Thus, the success of a model will again depend on the question of whether it focuses on explaining the variability of the real exchange rate through the nominal exchange rate and the price differences between countries of traded goods (first term on the right hand side of equation (2.4)) or on the variability of relative prices of traded and non-traded goods in the individual countries (second and third term on the right hand side of equation (2.4)). While Engel (1993) and Engel (1999) find evidence in favor of non-constant real exchange rates being mainly due to higher variability in relative prices between countries, with variability within countries found to be smaller, more differentiated results are found by other authors. Mendoza (2000) finds support for Engel's result only for periods in which the Mexican-US exchange rate was not actively managed. Chari et al. (2002) mostly confirm Engel's results. In contrast, Burstein et al. (2005) find evidence that the relative prices of traded and non-traded goods contribute significantly to real exchange rate variability when using an adapted measure for traded goods.

While this question has not been conclusively answered, the interest of this current chapter is to provide similar guidance to Engel (1993) now with regards to the real exchange rate based on wages:

$$RER^{WAGE\ INDEX} = \frac{W}{S \cdot W^*} \tag{2.6}$$

with W and W^* the wage indices in the home and foreign country and S the exchange rate (units of local currency per unit of foreign currency). Just as PPP holding would imply that the real exchange rate should be constant and equal to one, FPE would imply that the above wage-based real exchange rate should equal one. Analogously to Engel's (1993) result of $V(p_{ij}) < V(p_{ii^*})$ for prices (i.e. the variability V of prices p for different goods i, j within a country is lower then the variability V of prices p of identical goods i, i^* in different countries) we investigate the relationship of the intra-country variability $V(w_{ij})$ and inter-country variability $V(w_{ii^*})$ for wages. In line with the distinction in equations (2.2) and (2.4) between goods prices p_1 and p_2, and between traded and non-traded goods prices p_T and p_N, for which Engel employs both individual good's prices and consumer price subindices in his calculations, this chapter analyses both the variability between wages of specific professions, as well as variability in three pairs of wage subindices w_i and w_j:

Industrial versus service sector wages

Wage indices for these two sectors are calculated and their intra-country and inter-country variability is analysed, much in line with the traditional interpretation of traded versus non-traded goods being from the industrial and service sectors, respectively.

Wages in competitive versus uncompetitive professions

While mapping the industrial sector to traded goods and the services sector to non-traded goods is common in the literature, in terms of wages a clearer distinction might be made regarding competitive and uncompetitive wages. Many professions in the industrial sector will be exposed to international competition while most in the service sector will hardly be affected by international competition – but not all. For example, in the service sector the wages of call centre agents tend to be highly competitive internationally, while in the industrial sector a car mechanic might well not be exposed to international competition. Therefore, wage indices are constructed for competitive and uncompetitive professions, and their intra-country and inter-country variability is compared.

Wages for skilled versus unskilled professions

The intra-country and inter-country variability of relative wages between indices of skilled and unskilled professions is analysed.

Finally, the availability of profession-level wage data allows us to extend the analysis beyond the subindex level. Using profession-level panel data we control for fixed effects for individual professions, cities and years, before computing the variability of inter-country and intra-country relative wages. In contrast to Engel (1993) who analyses short- to medium-term variability, the focus of this study is on the longer-term variability of relative wages, as wages are likely to be significantly more sticky than prices, and as short-term deviations from constant real exchange rates can more likely be explained by factors such as investor flows and risk sentiment than by general equilibrium concepts.

This analysis is also informative with regards to the explanations of movements in the real exchange rates based on prices. For example, when testing for the Balassa-Samuelson effect on real exchange rates, researchers usually express the real exchange rate as a function dependent on the terms $(p_T - s - p_T^*)$, $(w_T - w_N)$ and $(w_T^* - w_N^*)$, as well as dependent on the differentials in productivity between the sectors and on the shares of production factors in the tradables and non-tradables sectors (see, for instance, Wagner, 2005, p. 4). Assuming competitive markets the real exchange rate in the tradables sector $(p_T - s - p_T^*)$ can be expressed as $(w_T - s - w_T^*) - (mpl_T - mpl_T^*)$ with mpl representing the logarithm of the marginal product of labour in the tradables and non-tradables sectors. Many empirical studies find that the Balassa-Samuelson assumptions of a constant real exchange rate in the tradables sector, and of wage equalisation between the tradables and non-tradables sectors do not hold. However, they do not usually examine which assumption has the more severe impact on the real exchange rate. This chapter therefore provides an indication as to whether the differential of wages between countries $(w_T - s - w_T^*)$ or the differential of wages within countries $(w_T - w_N)$ and $(w_T^* - w_N^*)$, are mostly responsible for movements in the real exchange rate in the context of the Balassa-Samuelson model.

2.4 Data Description

2.4.1 UBS Prices and Earnings Survey

For our comparison of intra- and inter-country variability of wages we employ wage data stemming from the UBS Prices and Earnings surveys. This data is described shortly in section 1.5 and in detail in appendix A. The characteristic of these wage data that make them appropriate for this analysis is their high global comparability and consistency over time. We use the survey measure "gross annual income" in constructing our wage indices.[8,9]

2.4.2 Data Strengths and Limitations

The key strength of these data is the identical definition of workers and professions across all cities, enabling the construction of consistent and comparable wage indices.[10] This is

[8] This measure is defined in the survey as "Gross annual income (sum of hourly, weekly or monthly earnings) taking into account family status and tax allowances including all fringe benefits such as profit participation, bonuses, vacation money, additional monthly salaries as bonus payments, allowances for children etc., but excluding overtime compensation."

[9] The data are provided in USD as converted from local currency using the average market exchange rate during the period of the survey.

[10] Note that the UBS Prices and Earnings survey does not provide country estimates, but only provides data for selected large cities. This ensures that the context of the professions is always comparable, e.g. it is not the case that the wage of a bus driver in a rural environment is compared to that of bus drivers in urban environments – the "large city" context is always the same for all data points. While many analyses compare countries rather than cities, for our purposes city data seems fully acceptable, as we are interested more in variability than in absolute levels.

essential as the FPE theorem implies only an equalisation of prices of *equivalent* production factors, i.e. of wages of workers with equivalent characteristics in equivalent jobs.[11] The condition for FPE that there should be no complete specialisation and thus that the same factors should be employed in both economies, is well reflected in this data, as only professions that exist in all large cities were included. The characteristic of data consistency and comparability is a significant advantage compared to using nearly any alternative wage data source that has a comparably long history: most wage data is compiled from a variety of different sources including each country's statistical offices, tax offices, labour unions etc. Wage indices constructed in this way, e.g. for the services or industrial sectors, will often reflect differing compositions of professions between countries, as not all professions exist in all countries – this frequently reduces the comparability of wage indices across countries. Also, with the UBS Prices and Earnings data we can determine the composition of the wage indices ourselves based on the wages of the professions surveyed, so we have transparency and flexibility in how we construct the wage subindices.[12] A second strength is the availability of wage data since the 1970s for selected emerging markets, which is of particular interest given recent sharp wage rises in some emerging markets, as these data are rarely available from alternative data sources.

In terms of limitations, the data do not reflect differences in the structure of labour markets, as household survey data would. This wage data therefore cannot be assumed to represent all professions within a city, but only certain professions, which exist in all cities. For our current application this limitation is not serious as we wish to compare only comparable production factors, i.e. workers with equivalent characteristics. A more serious limitation is the low data frequency. Due to the low frequency, these data will clearly not capture the short- or medium-term variability in intra- or inter country relative wages. This is a significant disadvantage and an annual or bi-annual frequency would have been preferred. However, as wages are likely to be more sticky than goods prices, a focus on the longer term variability of relative wages seems acceptable, compared to Engel's (1993) focus on short- to medium-term variations in relative prices. The low frequency of the UBS Prices and Earnings data does, however, pose additional challenges in the measurement of the variability, as is discussed in section 2.5.

[11] While this comparability in the definition of the professions and the jobs is a strength of the data, a fuller model would nonetheless aim to correct for productivity differentials. Lacking suitable productivity estimates on a subindex- and on profession-level for the time span under consideration, we refrain from attempting a productivity correction.

[12] As the wage subindices for industry and services, competitive and uncompetitive professions, and skilled and unskilled wages should be equivalent for all cities in this analysis, each city's index can clearly not reflect the structure of its labour market. Instead, our subindices are constructed as equally weighted means of the wages for the professions included in the specific subindex.

2.4.3 Alternative Data Sources

A dearth of good wage data at the industry or occupational level for a broad range of countries has significantly limited empirical research in this field. Alternative data sources, in particular Freeman and Oostendorp's *Occupational Wages around the World (OWW)* database were considered, but found to be less suitable for this application. While the frequency of the OWW database is higher than that of the UBS Prices and Earnings database and OWW covers more professions, the technical document to the OWW database (Oostendorp, 2005) reports that this data is cleaned and undergoes country-specific data type correction and standardisation procedures. A significant share of the values are imputed due to low response rates, they are based on different sources and are largely non-comparable (Oostendorp, 2005). While the standardisation procedures employed make this data useful and interesting in other applications, it is not considered suitable for this application due to the lack of comparability and the potential loss of variability during the standardisation procedures employed.

2.5 Methodology

2.5.1 Engel's Methodology for Prices

As in Engel's case it is not necessary for our purpose to develop a model based on economic theory to describe the relative wage processes (see Engel, 1993, p. 39). However, our analysis is different to Engel's in that we focus on longer-term variability: For one, wages are likely to be significantly more sticky than prices, and we consider short-term deviations from constant real exchange rates more likely to be explained by factors such as investor flows and risk sentiment. Furthermore, short- or medium-term variability is not captured in our data. Due to the low frequency of the data we cannot follow Engel's methodology of approximating the representation of the time series as an infinite-order autoregression based on Wold's Theorem (see Engel, 1993, p. 39) by estimating a 12th-order autoregression and then calculating the variance of the forecast error. However, Engel (1993) himself notes several difficulties in using this approach. Firstly, the tests on stationarity of the time series, necessary for the approximation by an AR(12), are inconclusive. Second, the F-test on equality of variances is known to be sensitive to the assumption that the underlying populations are normally distributed, and requires independence within and between the groups for which the variances are compared. As neither normality nor independence can be easily assumed, Engel does not attempt a formal statistical test, but indicates that if the conditions necessary for this modeling and test were fulfilled, the "null would be rejected in the vast majority of (...) case by case tests" (Engel, 1993, p. 41). We would add that Engel does also not address the problem of multiple comparisons or multiple testing.

2.5.2 Measuring Variability in Wages

Clearly, using an AR(12) as in Engel's case is unsuitable here due to the small sample size. In fact, no single method for measuring the variability of wages captures the different components of variability comprehensively in our view. We therefore employ several different approaches to examine the origin of the variability of the real exchange rate based on wages. In each case we compare the variability of relative wages within a country, denoted $V(w_{ij})$ with $w_{ij} = w_i - w_j$ (w_i and w_j can be the wages of two different professions, or of two different wage indices, e.g. skilled and unskilled wages), to the variability of an identical wage (either identical profession or identical wage index) between different countries, denoted $V(w_{ii*})$ with $w_{ii*} = w_i - s - w_{i*}$ following Engel's (1993) approach.[13]

Variance of Residuals from Regression Models

A very simple first approach is to estimate the variance of the residuals from a linear trend in the difference series w_{ij} and w_{ii*}, or from a regression model based on fractional polynomials. However, the standard regression conditions cannot be assumed to be met. Alternatively, in following with Engel's time series forecasting, a one-step forecast based on a constant could provide forecast errors for which the variances could be calculated and the F-test applied, if one were willing to assume that the conditions for the F-test (in particular normality, which cannot be tested reliably in small samples) were met. However, the variance (applied to the regression residuals in the first case and the forecast errors in the second case) is known not to be particularly stable for sample sizes smaller than 25 (see, for instance, Sachs and Hedderich, 2009, p. 184), and the power of such an F-test is very low. As an example, even if the ratio of variances of the residuals $V(w_{ii*})/V(w_{ij})$ equals two, for sample size 14 and $\alpha = 0.05$, the power would be only 32% in the one-sided test (see Lenth, 2006-9). In this chapter we will therefore investigate the variance of the residuals from a regression with a time trend in the difference series w_{ij} and w_{ii*}, but refrain from applying the F-test. This we do for the three sets of wage indices described in section 2.3 – industrial versus service sector, competitive versus uncompetitive, and skilled versus unskilled.

To control for city-specific, profession-specific or year-specific fixed effects we also compute variability based on wages at the level of individual professions (instead of

[13]Lower-case letters refer to natural logs.

subindices). For this we control for fixed effects in our wage data for individual professions, cities and years, before computing the variance of the difference terms of inter-country and intra-country residuals. We test various specifications including fixed effects, time trends and terms for city-year interaction. As this comparison is based on wages at the level of individual professions, unlike the other comparisons which are based on wage indices, the results are reported separately in section 2.6.2.

Median Absolute Deviation (MAD) from the Median

For the analysis based on wage subindices we also employ the scale parameter *MAD (Median Absolute Deviation from the median)* which is considered to be a good scale parameter for small sample sizes, and in samples with asymmetric distributions or potential outliers (cf. Sachs and Hedderich, 2009, p. 68).

The MAD scale parameter is impacted by a trend in the time series. The wage indices we employ trend upwards for most cities over time, as measured in nominal USD. The exceptions are countries which experience sharp and persistent currency volatility, and in particular depreciation versus the USD, such as Venezuela starting from its 1983 devaluation, or Mexico following its 1982 default on external debt leading to capital flight and devaluation, going into the early 1990s. Differencing two similarly trending wage indices as in our case ($w_{ij} = w_i - w_j$ and $w_{ii*} = w_i - s - w_{i*}$) reduces the impact of these trends, but does not eliminate them. We choose not to de-trend the series before applying MAD, so as to include a parameter that will reflect differences in trend in the variability measure.

Q^a_{adj} as Proposed by Gelper et al. (2009)

As a third measure of variability for wage subindices we employ the scale parameter Q^a_{adj} for univariate time series, which measures the median deviation from local linearity – in contrast to the global linearity which we use when calculating the variance of the errors from the regression model as described above (section 2.5.2). Q^a_{adj} is not affected by a linear trend in the data, and is robust as regards outliers (Gelper et al., 2009, p. 2). These characteristics stem from the use of vertical heights of triangles that are formed using consecutive data points – a necessary assumption is that each three consecutive data points can be approximated by a linear trend. Q^a_{adj} is an explorative method in the sense that it does not require an explicit modeling or regression fit of the underlying time series, nor of the variability process, which suits our application.[14]

[14]It is also suitable for online applications enabling instant updating, however, this characteristic is not required for our purposes. Instead, we calculate Q^a_{adj} only for the total time series.

When deriving influence functions and asymptotic variance of the estimators for a simple time series model with a level component and a random noise component Gelper et al. (2009, p. 4) assume independence of the noise components. However, their simulation experiments confirm that the relative performance of the estimators is maintained also for dependent errors. For the parameter a we select $a = 0.5$ which provides a good trade-off between robustness and efficiency, see Gelper et al. (2009, p. 17). The measure is efficient both in the limit and at a finite sample level and has good small sample properties as already for 20 observations Gelper et al. (2009) find that the asymptotic behaviour is closely approximated for the Q^a_{adj} parameter.

2.5.3 Hypothesis Testing

Engel (1993) refrained from carrying out a formal statistical test as conditions for the test could not be shown to be met. Neither is the current setup with wages (instead of prices) suited to hypothesis testing, with the most significant hurdles being the difficulty in testing the normality of the underlying distributions in small sample sizes, and the lack of independence between the intra- and inter-country differences. Additionally, the multiple testing setup means that it must be expected that the null hypothesis can be falsely rejected in a certain number of cases. Recognizing these limitations we focus on the descriptive results of the question whether $V(w_{ii*})$ is larger or smaller than $V(w_{ij})$ in section 2.6 as Engel did, and only shortly provide the results of the tests described below, whose validity would only be given if it were assumed that all necessary conditions are met.

Dixon and Mood's Statistical Sign Test The statistical sign test of Dixon and Mood (1946) evaluates the relative frequency of one scale parameter being larger than the other, and vice versa. The null hypothesis of the statistical sign test is that the differences of paired observations (here paired scale parameters) are on average not different from zero. Stated differently, the null hypothesis is that the distribution of the signs of the differences has zero median. The test statistic is

$$z = (|n - 2h| - 1)/\sqrt{n} \tag{2.7}$$

with n the number of comparisons and h the (absolute) frequency of the less common sign. For sample sizes above 40 the asymptotic efficiency of the test starts to fall. When approximating the binomial distribution with the standard normal distribution the test statistic for the one-sided test with null hypothesis $V(w_{ii*}) < V(w_{ij})$ must be larger than 1.6449 to reject the null hypothesis for $\alpha = 0.05$, where h is the number of times that

$V(w_{ii*}) < V(w_{ij})$.

For the wage subindex data we apply this test for the three scale parameters: the variance of the regression residuals, MAD and Q^a_{adj}. Importantly, however, the assumption of independence between the paired observations is not given in this context.

Levene Test in the Version of Brown-Forsythe The second approach is the Levene test (Levene, 1960) for homogeneity of variances in the version of Brown-Forsythe as implemented in the R-package *lawstat*.[15] This test is based on the ANOVA statistic which is applied to the *median absolute deviation (MAD)* which, as mentioned above, is considered to be a good scale parameter for small sample sizes, and in samples with potential outliers (see Sachs and Hedderich, 2009, p. 68). In the Brown-Forsythe version (mean replaced by median compared to the original Levene test) the test proves robust when used for non-normal data and still retains good statistical power. The test can be used from sample sizes as small as 10, so that the low frequency of the UBS Prices and Earnings data does not pose additional problems.

Fligner-Killeen Test The Fligner-Killeen test for homogeneity of variances as implemented in the R-package *stats*[16] is known from simulation studies as particularly robust with regards departures from normal distributions (see Conover, Johnson and Johnson, 1981). As implemented in the R-package *stats* the test uses median centering.

2.5.4 Methodology: Additional Remarks

For 34 cities suitable data is available.[17] Athens is omitted in spite of having been included in each survey as for some years important wage data is missing, resulting in $34 * 33 = 1122$ comparisons being calculated. In the US, Switzerland, Brazil and Canada data for two or three cities is available. As we here wish to focus on pairwise comparisons between countries with different currencies, pairs of cities within one country are removed (12 pairs).

While for a large part of the period 1970 to 2009 nominal exchange rates for most currency pairs were allowed to float, the clear regional exception is Europe. After the end of the Bretton Woods system of fixed exchange rates on August 15, 1971, many European nations decided to limit currency volatility first through the European "currency snake", later within the framework of the European Monetary System and finally by adopting the euro. Therefore, we also exclude pairwise comparisons which involve two euro countries.

[15] Sourced from R Core Team (2011-2013).

[16] Sourced from R Core Team (2011-2013).

[17] See table A.3 in the appendix for details on the inclusion of cities in the surveys over time.

As the Danish krone has been closely linked to the euro, it is treated equivalently to euro countries. Omitting these 110 pairs within or closely linked to the Eurozone, leaves 1000 city pairs for which we report results.

We first analyse the more aggregated level of wage subindices, running comparisons for intra- and inter-country wage variability for the following pairs of wage subindices and their reverse constellations:

1. Industrial versus service sector
2. Competitive versus uncompetitive professions
3. Skilled versus unskilled professions

We verify the results for the case that the composition of the wage indices is held exactly identical from 1970 to 2009, i.e. only professions are included in the wage indices for which data is available for all survey years. The advantage is that the indices are more consistent over time; the disadvantage is that the indices in this case are based only on a few professions and potential outliers might have a stronger impact. Additionally we run the calculations on the dataset including data only from 1976 onwards due to the distorting effects high exchange rate volatility in the early 1970s could have had. For this case we again maintain consistent index compositions over time, including only those professions in the wage indices for which data is available in all survey years from 1976 to 2009.

Finally, we provide results based on the variability on wages of individual professions (as compared to the above wage subindices), after having corrected for fixed effects, time trends and city-year interaction. The results are presented in the next section.

2.6 Results

2.6.1 Results for Wage Subindices

Descriptive Results

We compare the variability of the difference between log wages of an identical wage index between two cities ("inter-country"), denoted $V(w_{ii*})$, with the variability of the difference of differing wage indices within the cities ("intra-country"), denoted $V(w_{ij})$. An overview of the results of the analysis of the wage subindices is shown in table 2.1. The results indicate that for all three measures of variability (the variance of the residuals from the linear time trend regression, the median absolute deviation from the median (MAD), and Gelper et al.'s Q^a_{adj}) in the vast majority of comparisons $V(w_{ij}) < V(w_{ii*})$.[18] Viewed

[18]An example of more detailed results indicating actual variability measures is given in table C.1 in the appendix.

over all wage subindices and all variability measures, $V(w_{ii*}) < V(w_{ij})$ in only 12.0% of comparisons. Comparing the different variability measures across all wage subindices, $V(w_{ii*}) > V(w_{ij})$ in 93.3%, 87.7% and 82.9% of comparisons for the MAD, Q^a_{adj} and residual variance measures, respectively.

Table 2.1: Summary of results for variability comparisons for all pairs of cities for which suitable data are available. For each of the three pairs of wage indices and their reverse constellations the variability is calculated for each of the three variability measures. All wage data for the relevant cities from the surveys from 1970 to 2009 are included.

Wage variability compared	# of comparisons	# of $V(w_{ij}) < V(w_{ii^*})$			% of $V(w_{ij}) < V(w_{ii^*})$		
		ResVar	MAD	Qadj	ResVar	MAD	Qadj
All wage comparisons	6000	4974	5598	5262	82.9%	93.3%	87.7%
Inter-country industry, vs services	1000	886	948	933	88.6%	94.8%	93.3%
Inter-country services, vs industry	1000	812	939	901	81.2%	93.9%	90.1%
Inter-country competitive, vs uncompetitive	1000	829	911	914	82.9%	91.1%	91.4%
Inter-country uncompetitive, vs competitive	1000	724	914	890	72.4%	91.4%	89.0%
Inter-country skilled, vs unskilled	1000	860	934	770	86.0%	93.4%	77.0%
Inter-country unskilled, vs skilled	1000	863	950	856	86.3%	95.0%	85.6%

The tables C.2 and C.3 in the appendix show the equivalent results when omitting professions not included in all survey years, and when using only the data from 1976, again holding the composition of the wage indices constant, respectively. The previous results are confirmed, in fact, viewed over all wage subindices and all variability measures, $V(w_{ii*}) < V(w_{ij})$ in only 8.8% and 9.2% of comparisons, respectively, versus 12.0% when using all wage data. Comparing the different variability measures across all wage subindices when holding the composition of the wage indices constant from 1970 (from 1976), $V(w_{ii*}) > V(w_{ij})$ in 94.5% (94.8%), 91.6% (91.8%) and 87.6% (85.8%) of comparisons for the residual variance measures, MAD and Q^a_{adj} measures, respectively.

Looking more broadly at the distribution of the ratios of $V(w_{ii*})/V(w_{ij})$, the histograms shown in figures C.1 to C.6 in the appendix show that – viewed over all wage subindices and all variability measures and now again including data from all professions – $V(w_{ii*})/V(w_{ij})$

takes on values between 1 and 2 in 30.3% of comparisons. In nearly 60% of cases the inter-country variability is more than double as high as the intra-country variability, i.e. $V(w_{ii*})/V(w_{ij}) > 2$. Comparing the different variability measures over all wage subindices, the ratio $V(w_{ii*})/V(w_{ij})$ takes on values between 1 and 2 in 28.1%, 35,0% and 27.8% of comparisons, for MAD, Q^a_{adj} and the residual variance measure, respectively. Therefore, the MAD parameter produces the highest percentage of cases in which the inter-country variability is more than double as high as the intra-country variability, at 65.2%, with equivalent values of 52.8% for Q^a_{adj} and 55.1% for the variance of the residual.

When holding the composition of the wage indices constant from 1970 (from 1976), $V(w_{ii*})/V(w_{ij})$ takes on values between 1 and 2 in 26.8% (27.3%) and $V(w_{ii*})/V(w_{ij}) > 2$ in 64.4% (63.4%) of comparisons – viewed over all wage subindices and all variability measures. Comparing the different variability measures over all wage subindices, the ratio $V(w_{ii*})/V(w_{ij})$ takes on values between 1 and 2 in 31.6% (33.6%), 32.7% (30.8%) and 16.2% (17.4%) of comparisons, for MAD, Q^a_{adj} and the residual variance measure, respectively. The equivalent values for $V(w_{ii*})/V(w_{ij})$ larger than 2 are 60.0% (58.2%), 54.9% (55.1%) and 78.3% (77.0%). In these cases the residual variance measure produces the highest share of comparisons in which the inter-country variability is more than double as high as the intra-country variability.

These descriptive results point in the same direction for FPE as Engel's (1993) results pointed to for PPP: For all three measures of variability the vast majority of comparisons indicate that the variability of identical wage indices between different countries is higher than the variability of wage indices for different sectors within one country.

Hypothesis Testing

While recognizing the caveats discussed in section 2.5.3 with regard to hypothesis testing in the current context, we provide some results below.

Results: Dixon and Mood's Statistical Sign Test This test is performed for each city with all of its valid inter-country pairs for each of the three selected variability measures (the variance of the residuals from the linear regression, the median absolute deviation from the median (MAD), and Gelper et al.'s Q^a_{adj}). For each variability measure tables C.4 to C.21 in the appendix show the detailed results for the three pairs of wage indices selected (industry versus services, competitive versus uncompetitive and skilled versus unskilled, and their reverse constellations), using the data from all professions and all years. Table 2.2 provides a summary.

Table 2.2: Summary of results of Dixon and Mood statistical sign test for each city with all of its valid inter-country pairs, using data from all professions.

Dixon & Mood test H0: $V(w_{ij}) > V(w_{ii^*})$	H0 rejected, $\alpha = 0.05$ (no multiple correction) % of cities			H0 rejected, $\alpha = 0.05/34$ (Bonferroni correction) % of cities		
Variability measures	ResVar	MAD	Qadj	ResVar	MAD	Qadj
All wage comparisons	$\frac{169}{204}$=83%	$\frac{197}{204}$=97%	$\frac{181}{204}$=89%	$\frac{141}{204}$=69%	$\frac{185}{204}$=91%	$\frac{167}{204}$=82%
Inter-country industry, vs services	$\frac{32}{34}$=94%	$\frac{33}{34}$=97%	$\frac{33}{34}$=97%	$\frac{28}{34}$=82%	$\frac{32}{34}$=94%	$\frac{32}{34}$=94%
Inter-country services, vs industry	$\frac{27}{34}$=79%	$\frac{34}{34}$=100%	$\frac{31}{34}$=91%	$\frac{20}{34}$=59%	$\frac{31}{34}$=91%	$\frac{28}{34}$=82%
Inter-country competitive, vs uncompetitive	$\frac{28}{34}$=82%	$\frac{30}{34}$=88%	$\frac{31}{34}$=91%	$\frac{24}{34}$=71%	$\frac{27}{34}$=79%	$\frac{29}{34}$=85%
Inter-country uncompetitive, vs competitive	$\frac{21}{34}$=62%	$\frac{32}{34}$=94%	$\frac{31}{34}$=91%	$\frac{16}{34}$=47%	$\frac{29}{34}$=85%	$\frac{27}{34}$=79%
Inter-country skilled, vs unskilled	$\frac{30}{34}$=88%	$\frac{34}{34}$=100%	$\frac{25}{34}$=73%	$\frac{26}{34}$=76%	$\frac{32}{34}$=94%	$\frac{23}{34}$=68%
Inter-country unskilled, vs skilled	$\frac{31}{34}$=91%	$\frac{34}{34}$=100%	$\frac{30}{34}$=88%	$\frac{27}{34}$=79%	$\frac{34}{34}$=100%	$\frac{28}{34}$=82%

Using the standard normal approximation for the binomial distribution the one-sided statistical sign test of the null hypothesis $V(w_{ii*}) < V(w_{ij})$ over all wage comparisons is rejected at the 5% level if the test statistic is larger than 1.64. This is the case for 83% of cities when using the variance of the residual as scale parameter, 97% for MAD and 89% for Q^a_{adj}. Adjusting for the multiple testing problem using the Bonferroni correction method, $\alpha = 0.05$ is replaced by $\alpha = 0.05/34 = 0.00147$. Therefore, again using the standard normal approximation for the binomial distribution, the null hypothesis for the one-sided test is rejected if the test statistic is larger than 2.97. The equivalent numbers after Bonferroni correction due to multiple testing are 69%, 91% and 82%. The fact that H0 is rejected more frequently for the MAD parameter (the only one of the three scale parameters that also measures the variability caused by linear trends in the data) indicates that linear trends in wages are likely more similar between sectors within a city than between identical sectors between cities in different countries, therefore further reinforcing the higher variability between cities already measured by the other scale parameters. In contrast, the lowest share of rejections is measured when using the variance of the residual as the scale parameter, and in particular when comparing wages in uncompetitive professions between cities versus the variability of the indices of competitive versus uncompetitive wages within a city. These low rejection levels (62%, and 47% with Bonferroni correction) might at first seem surprising, as one would expect uncompetitive wages to nonetheless follow competitive wage trends within a city, as professions in uncompetitive sectors would else become unattractive. However, it is likely that the high proportion of public sector professions in the uncompetitive wage index (e.g. bus drivers, school teachers), result in unusually low volatility, therefore these results do not undermine the broader picture of high levels of rejection of the null hypothesis.

The cities for which the null hypothesis $V(w_{ii*}) < V(w_{ij})$ is rejected least often are Paris, Madrid and Bogota. The null hypothesis is always rejected for Buenos Aires, London, Mexico City, Tokyo and Toronto. Interestingly, there seems to be no systematic difference in levels of rejection between cities in developed and emerging countries. While these test results look convincing at first sight they cannot be assumed to be statistically valid due to a lack of independence of the individual comparisons, as previously noted.

When holding the composition of the wage indices constant from 1970 these results are confirmed. The one-sided statistical sign test of the null hypothesis $V(w_{ii*}) < V(w_{ij})$ over all wage comparisons is rejected at the 5% level (rejected at the 5% level after Bonferroni correction) for 96% (93%), 95% (87%) and 89% (85%) of cities for the variance of the residual, MAD and Q^a_{adj}, respectively.[19] When using the data from 1976 onwards and holding the composition of the wage index constant similar results are achieved: The null hypothesis $V(w_{ii*}) < V(w_{ij})$ over all wage comparisons is rejected at the 5% level (rejected at the 5% level after Bonferroni correction) for 97% (94%), 94% (89%) and 85% (79%) of cities for the variance of the residual, MAD and Q^a_{adj}, respectively.[20]

These results again point in the same direction as Engel's (1993) results: For all three measures of variability the clear majority of comparisons reject the null hypothesis of $V(w_{ii*}) < V(w_{ij})$.

Results: Levene Test for Homogeneity of Variances The Levene test as implemented in the R-package *lawstat* in the version of Brown-Forsythe is performed on each valid pair of cities. The results are reported in tables C.54 to C.59 in the appendix. The results do not confirm the descriptive results of section 2.6.1 nor the Dixon and Mood test results of section 2.6.1, but also do not contradict them: the one-sided test of the null hypothesis $V(w_{ii*}) < V(w_{ij})$ is rejected for 3187 out of the 6000 comparisons (1000 city pairs and three pairs of wage indices and their reverse constellations), i.e. 53% at the 5% level. However, adjusting for multiple testing using the Bonferroni method (i.e. α is adjusted to 0.05/1000=0.00005 as in this case it is more appropriate to test each of the

[19]Table C.22 in the appendix provides a summary of the results, while tables C.23 to C.34 provide detailed results.

[20]Table C.35 in the appendix provides a summary of the results, while tables C.36 to C.53 provide detailed results.

1000 pairs of cities individually, and not as in the Dixon and Mood test in 34 groups) results in only 82 rejections of the null hypothesis for the 6000 comparisons. Using less conservative multiple correction methods such as Simes-Hochberg does not change this result significantly. When limiting the data to the professions available since the first survey in 1970 the results are similar: the one-sided test of the null hypothesis $V(w_{ii*}) < V(w_{ij})$ is rejected for 2638 out of the 4000 comparisons (competitive/uncompetitive wage indices not used due to limited data on competitive professions in the first surveys), i.e. for 66% at the 5% level, and for only 146 comparisons or 3.6% after Bonferroni correction. When using the data from 1976 onwards and holding the composition of the wage indices constant the one-sided test of the null hypothesis $V(w_{ii*}) < V(w_{ij})$ is rejected for 3690 out of the 6000 comparisons, i.e. for 61.5% at the 5% level, and for only 95 comparisons or 1.6% after Bonferroni correction.

Clearly, the Levene test for homogeneity of variances does not confirm nor contradict the findings of the Dixon and Mood statistical sign test and of the descriptive analysis that $V(w_{ii*}) > V(w_{ij})$.

Results: Fligner-Killeen Test We additionally use the Fligner-Killeen test for homogeneity of variances as implemented in the R-package *stats* due to its robustness with regards departures from normal distributions, which is likely to be problematic in the current context. However, here too the results are inconclusive and broadly similar to those of the Levene test (see tables C.60 to C.65 in the appendix). The one-sided test of the null hypothesis $V(w_{ii*}) < V(w_{ij})$ is rejected for 3269 out of the 6000 comparisons, i.e. 54% at the 5% level. Adjusting for multiple testing using the Bonferroni method results in only 15 rejections of the null hypothesis for the 6000 comparisons. 64% (59%) of comparisons reject the null when data is limited to the professions included in all surveys (data is limited to 1976-2009 with unchanged composition of indices), falling to 1.5% (1.0%) after Bonferroni correction.

Therefore, as with the Levene test, the Fligner-Killeen test does not confirm nor contradict previous results.

2.6.2 Results for Profession-Level Wage Data (no wage subindices)

We make use of the profession-level wage data available to extend the analysis beyond the subindex level. Using profession-level panel data allows us to control for fixed effects for individual professions, cities and years, before computing the variability of inter-country and intra-country relative wages. We test various specifications including fixed effects, time trends and terms for city-year interaction. Tables C.66 and C.67 in the appendix show the results for the specifications including city, year and profession fixed effects, and these same fixed effects adding city-year interaction terms, respectively.[21] Overall, the results when controlling for different fixed effects, for a time trend and for city-time interaction terms remain very similar. These results confirm the findings in section 2.6.1, and are even more clear-cut: The variability of the difference in wages of identical professions between different countries $V(w_{ii*})$ is on average higher than the variability of wages in different professions within one country $V(w_{ij})$ on average for all cities for all equation specifications that correct for one or more fixed effects (city, profession and/or year). This is also the case when time trends are additionally controlled for. Only in the specifications controlling additionally for city-year interactions does this not quite hold: The mean inter-country variability is not larger than mean intra-country variability for two out of 34 cities (Bogota and Hong Kong).

Looking at the distribution of ratios of inter-country to intra-country variability we see that when controlling for city, year and profession fixed effects only for one fifth (21%) of cities is this ratio between one and two – i.e. in 79% of cases $V(w_{ii*})$ is more than twice as large as $V(w_{ij})$. When controlling additionally for city-year interaction terms the ratio was larger than two in 59% of cities, between one and two in 35%, and below one in only 6% of cities. Results for the selected specifications are shown as histograms in figures C.7 and C.8 in the appendix. Due to the stated limitations on hypothesis testing, we do not report a formal test as was done for the calculations based on wage subindices in the previous sections. Nonetheless, the profession-level data corrected for fixed effects, time trends and city-year interactions clearly confirm the results in section 2.6.1 using wage subindices.

2.7 Conclusion

FPE theory implies that under certain conditions trade in goods can act as a substitute for factor mobility, implying that wages will converge even without factor flows between countries. However, empirical evidence is mixed and FPE theory seems at odds with large and persistent divergences in international wages. This chapter contributes to explaining deviations from FPE by drawing a parallel to the analogous "price" problem and Engel's (1993) decomposition of the real exchange rate. By decomposing an adapted expression for the real exchange rate, now based on factor prices (wages), instead of goods prices, we

[21] The results shown exclude professions that were included in the surveys only after 1976 to ensure availability of at least twelve panel observations per profession. Adding professions with fewer observations did not materially change the outcome and the results are not reported. As with previous results, the results shown here exclude pairs of cities within the same country and pairs with closely linked currencies in Europe.

investigate whether deviations from FPE have historically been better explained by the variability in the wages between countries, or by the variability of wages within countries, thereby providing an empirical yardstick for models of international factor prices.

Using a number of different wage indices and three different measures for variability, as well as profession-level wage data corrected for fixed effects, our results show that historically the variability of identical wage indices between countries has been higher than the variability of different wage indices within a country. This result is analogous to Engel's early result, and indicates that deviations from FPE are more likely driven by the higher variability of wages between countries, than by the variability of different wages within countries. As in Engel's analysis this result is, however, based primarily on descriptive statistics as numerous hurdles put into question the validity of statistical hypothesis testing. Interestingly, there is no clear evidence of structural differences in the wage comparisons involving only developed markets and those involving also wages in emerging markets.

With regards to the traditional analysis of the real exchange rate this analysis is informative in that it shows that the Balassa-Samuelson assumptions of a constant real exchange rate in the tradables sector, and of wage equalisation between the tradables and non-tradables sectors have a differing importance with regard to movements in the real exchange rate. While the literature mostly confirms that these assumptions do not hold empirically, our analysis points to a larger impact on the real exchange rate likely stemming from the movements in the real exchange rate of tradables $(p_T - s - p_T^*) = (w_T - s - w_T^*) - (mpl_T - mpl_T^*)$ and only to a lesser extent from the lack of equalisation of wages within countries in $(w_T - w_N)$ and $(w_T^* - w_N^*)$. Broadly speaking this analysis is in line with the Balassa-Samuelson projections in that it finds more homogeneity of wages within a country, than between countries. It emphasizes that models of international factor prices explaining deviations from FPE should encompass explanations for the large differences and variability in wages of equivalent workers in equivalent jobs across different countries, and offer explanations of much smaller variability in differences between professions' wages within a country.

Chapter 3

The Effect of India's Economic Liberalisation on Urban Wage Inequality: Evidence from a Synthetic Control Approach

3.1 Introduction

In this third chapter, which is based on Boes and Weisser (2012), we analyse how wages for different skill levels were affected by policy reforms in the context of ongoing globalization using India, and the city of Mumbai, as a case study.

Far-reaching economic liberalisation started after India's 1991 balance-of-payments crisis. The reforms marked a turning point in India's post-colonial, largely socialist-style state-directed economy towards a market economy. They included increasing openness towards international trade and investment through a sharp reduction in tariffs and non-tariff barriers, deregulation, privatisation, and reforms in taxation policy. The reforms are credited with having lifted about 300 million people out of extreme poverty by substantially increasing economic growth. Real GDP per capita, as a measure of average well-being, rose by around 44% between 1991 and 2001 according to IMF data. While the success of the reforms in terms of economic growth is widely accepted, studies of the impact on the income distribution and skill premia have shown diverging results. In particular, some measures point to a marked increase in inequality, for example, Cain et al. (2010) calculate an increase of India's Gini coefficient of about 4 percentage points between the 1980s and

2004 levels. This gives rise to concerns that the economic gains from the reforms might have disproportionately benefitted the better-off.

In this chapter, we look at the effect of the reforms on the development of skill premia. Using data from the UBS Prices and Earnings surveys from 1982 to 2009 and Mumbai as a case study, we seek to compare relative wages with and without the reforms. This comparison, while theoretically sound, suffers from a fundamental identification problem: the trends in skill premia if no economic reforms had taken place – and India had been subjected only to ongoing external globalization trends – is a counterfactual outcome, and is thus unobserved. However, India's liberalisation provides a good example of a natural experiment. First, the reforms were not generally anticipated by politicians, the general public or international investors, as the balance-of-payments crisis escalated suddenly, providing clear pre- and post-treatment periods.[1] Secondly, the reforms were mandated by the IMF as a condition for the bailout and were thus externally determined and less prone to distortions by politicians and interest groups.

In order to address the issue of missing counterfactual wage trends we employ the method of synthetic control, as proposed by Abadie and Gardeazabal (2003) and extended in Abadie et al. (2010). We analyse the skill premia in the city of Mumbai due to its important role as a regional trading and economic hub. We use a broad group of 35 other large cities as the donor pool, including many trading centres in emerging markets, to construct the synthetic control. As cities will tend to be affected differently by liberalisation than rural areas we prefer to compare Mumbai to other global hubs than to country data which can include large rural/agricultural populations. Also, the development in other cities internationally is likely to provide a good counterfactual representation of general globalization trends as compared to the specific effects brought on by India's internal reforms.

The remainder of the chapter is structured as follows: Section 3.2 reviews the related theoretical and empirical literature on the economic impact of liberalisation in India. Section 3.3 describes the data, and a brief explanation of the synthetic control method is given in section 3.4. The estimation results are presented in section 3.5, and several robustness checks are provided. Section 3.6 discusses the implications of our results, and section 3.7 concludes.

[1] While the 1980s did see some limited economic reforms in India they are not comparable in depth or scope to the reforms of the 1990s.

3.2 The Impact of Trade and Liberalisation on Wage Inequality

3.2.1 Theoretical Considerations

Conventional trade theory, which combines the Heckscher-Ohlin framework with the Stolper-Samuelson theorem, provides a first guideline as to how one could expect trade and globalisation to impact wage inequality. In the Heckscher-Ohlin framework (with two countries, two production factors and two goods) the country in which capital (labour) is relatively more abundant will specialise on and export more capital- (labour-) intensive goods. The Stolper-Samuelson theorem provides the link between product prices and wages: The price of both goods will converge to a world price, which is higher (lower) than the original price in the country which is relatively abundant (less abundant) in the factor the good is intensive in. Therefore, the prices of factors that are intensive in export goods tend to rise, while those intensive in import goods tend to fall. Under the assumption that developed countries are relatively more abundant in capital and skilled labour, and developing countries are relatively abundant in unskilled labour, this factor-endowment framework suggests that the former countries will see wage inequality rise, while the latter would see it fall when trade is introduced or increased.

As described by Wood (1997) this framework is largely coherent with the experience of many of the newly industrialised East Asian economies including Hong Kong, Korea, Singapore and Taiwan from the mid-1960s until the 70s. However, empirical studies of the effects of increased globalization and trade on the income distribution for developing nations since the 1980s mostly point in the opposite direction. For example, Cragg and Epelbaum (1996) provide evidence of sharply rising skill premia in Mexico. Increasing returns to skill are also identified by Attanasio et al. (2004) for Columbia and by Galiani and Sanguinetti (2003) for Argentina.[2]

These mixed results from empirical studies in emerging markets have led to a search for explanations for the apparent link between economic opening and a rise in wage inequality. A first line of argumentation extends the Heckscher-Ohlin framework by endogenous technological change. Through increased international trade developing countries are likely to acquire more advanced production technologies, increasing the relative demand for skilled versus unskilled labour (Bound and Johnson, 1992; Robbins, 1996; Wood, 1997). This trade-induced skill-biased technological change (SBTC) is reinforced when production liberalisation increases foreign direct investment (Berman et al., 1998), or when multinational companies establish a presence in formerly closed economies. Acemoglu (2002, 2003) develops a model of endogenous technological change whereby skilled labour's

[2]See Goldberg and Pavcnik (2007) for a comprehensive review of this literature.

increasing demands for technologically advanced goods induces SBTC through the market size effect. Although there is no clear consensus among researchers, SBTC is widely considered a key driver of increased wage inequality in developing countries over the past decades. Feenstra and Hanson (1996) argue that a relative increase in the demand for skilled labour in developing economies is reinforced by the trend towards trade in intermediate products, also referred to as "production sharing" or "outsourcing".

A second line of argumentation is based on the idea that global production sharing induces workers to lose bargaining power in wage negotiations through the threat of outsourcing, especially if they are unskilled. Evidence of this is presented by Choi (2006), who relates greater international openness to the declining power of trade unions. In this sense, trade liberalisation can function analogously to an increase in the size of the labour supply pool, particularly for unskilled workers. In the Heckscher-Ohlin framework this would apply to unskilled labour in the developed country, see for example Geishecker (2006), who finds that outsourcing to Central and Eastern Europe has reduced the demand for manual workers in Germany. This framework assumes trade to be between a developing and a developed country, so-called North-South trade. But trade is also taking place between countries abundant in unskilled labour, i.e., South-South, which does not imply the gains to unskilled labour in developing countries expected in the traditional Heckscher-Ohlin framework. Gourdon (2011) asserts that increasing wage inequality in some developing countries is related to growing South-South trade, i.e., trade between countries abundant in unskilled labour.

A third line of argumentation weakens the Heckscher-Ohlin assumption of perfect competition. Industry wage premia which prior to liberalisation are sustained through trade protection, imperfect competition or limited factor mobility might not be viable in the face of post-liberalisation international competition. Tariff reductions could reduce profits of previously protected firms thereby leading to falling industry wages, see e.g., Revenga (1997). Thus, patterns of protection prior to liberalisation can differ from the Heckscher-Ohlin framework assumptions. Goldberg and Pavcnik (2004, p. 11-13) review the literature for Latin America and unwind the seeming inconsistency for some developing countries: As unskilled labour-intensive industries in Latin America were often protected with the highest tariffs prior to trade liberalisation and therefore experienced the highest tariff reductions during reform, these sectors' product price adjustment was downwards, making the increase in wage inequality observed consistent with the Stolper-Samuelson theorem. Differing degrees or speeds of liberalisation across sectors can have a similar distorting impact on wage inequality.

Finally, tariff reductions can lead to productivity improvements in developing countries, and gains might be shared with workers. “Defensive innovation” enables companies to keep up with increased competition (Wood, 1995; Thoenig and Verdier, 2003), in particular if they are close to the technological frontier of their industries (Aghion et al., 2001). Depending on whether the industries achieving the strongest productivity improvements employ more skilled or unskilled labour, this can shift the income distribution. More broadly, wage inequality might be assumed to be affected through the growth channel, e.g., through higher growth based on productivity improvements. However, the relationship between trade liberalisation and economic growth remains controversial, as does the postulated Kuznets effect of growth on inequality: Most empirical studies reject the Kuznets hypothesis that economic growth will at first reinforce wage inequality due to the necessary restructuring of the economy, but subsequently led to a reduction in inequality (Ravallion, 1995; Deininger and Squire, 1998).

3.2.2 Evidence of the Impact of India’s Liberalisation on Wage Inequality

As the above review indicates the effects of economic liberalisation can be manifold and depend on a country’s very specific situation. For India the debate on poverty and inequality is at times fierce, in part due to the controversial quality of data sources.[3] Measures of consumption in India’s National Sample Survey (NSS) show a wide gap to National Accounts statistics, and this differential has grown over the past years. Further, the NSS data captures only a small share of private consumption and likely underreports consumption of the wealthy, but probably is the most accurate source of data for the consumption of the poor. Tax return data, as used for example by Banerjee and Piketty (2005), are thought to better capture income in the upper part of the distribution, though this too is controversial. Substantial differences in trends between states, between the urban and rural populations, and between the services, manufacturing and agricultural sectors makes providing generalised results for India even more complex (see Topalova, 2007; Tiwari, 2010).

Skill-biased technological change (SBTC)

A number of studies find evidence of SBTC in post-liberalisation India. Kijima (2006) finds that wage inequality in India as measured by the 90th versus the 10th percentiles of the wage distribution started increasing in the 1980s, but the increase accelerated in the post-reform 1990s due to an increasing demand for skilled labour. The study finds

[3] See Deaton and Kozel (2005) for a review of this debate.

that SBTC (measured as within-industry demand shifts) was the main driver for this acceleration, whereas trade reforms (measured by between-industry shifts of skilled workers) were not an important contributor. Banga (2005) uses cross-industry panels from the Indian manufacturing sector and similarly finds SBTC (and higher FDI) to have pushed up inequality (while trade is here found to have actually reduced it). In contrast, Srivastava and Mathur (2011) using translog cost function analysis find that both trade and technology have tended to increase inequality. Chamarbagwala (2006) uses wages of college versus non-college educated workers as a measure for inequality and finds that skill-upgrading *within* industries was the main driver for the rising demand for skilled labour, and that domestic liberalisation and reforms to the external sector failed to create work for unskilled labour. Ramaswamy (2008) and Sen (2008) find a relative increase in employment and wages of skilled labour consistent with SBTC in the 1990s. However, Sen (2008) also points to the industry structure of protection pre-liberalisation as a source of increasing inequality, as discussed in the next section.

Industry wage premia

The second important channel through which trade and liberalisation can affect inequality is industry wage premia. Both Dutta (2007) and Kumar and Mishra (2008) studied wages in the Indian (urban) manufacturing sector from 1983 to 1999, but their results diametrically oppose each other. Dutta (2007) finds that after controlling for worker characteristics higher tariffs were indeed related to higher industry wage premia, and that industries in which tariffs were reduced experienced falling industry wage premia. As initial levels of trade protection were on average higher for low-wage sectors, and tariffs were reduced most in these sectors, Dutta (2007) concludes that at least some of the increases in India's wage inequality were due to trade liberalisation. In contrast, Kumar and Mishra (2008) find that high trade protection in a sector was related to *lower* wages in that sector and that wages increased disproportionately in sectors experiencing the sharpest tariff reductions. This result is consistent with liberalisation-induced firm level productivity gains shared with the workers. As tariff reductions were larger in sectors employing a larger share of unskilled labour, Kumar and Mishra (2008) suggest that trade liberalisation has actually lead to a reduction in India's wage inequality.

Service sector reforms versus trade liberalisation

Cain et al. (2010) and Mehta and Hasan (2012) point to an important weakness in many studies on wage inequality in India: that they have tended to focus on manufacturing while ignoring India's large services sector. Cain et al. (2010) use inequality decompositions to show that a large share of the rise in consumption of more skilled households between 1993 and 2004 stem from their incomes derived in the services sector. This sector too underwent huge reforms as the 1990s trade opening was accompanied by a raft of service sector liberalisation measures. In fact, with regard to their impact on wage inequality, Mehta and Hasan (2012) find "that effects of services reforms are many times larger than those of trade liberalisation". However, they also estimate that 30% to 66% of the rise in wage inequality can be empirically explained neither by tradables nor by non-tradables liberalisation.

Economic opening and bargaining power of unions

Finally, some studies point to the falling bargaining power of unions as a potential source of increased wage inequality. For example, Mathur and Mishra (2007) confirm membership declines and a reduction in bargaining power of unions. While official data on the union density is considered suspect (Das, 2000), a number of sources indicate that union declines began in the 1990s (Kuruvilla et al., 2002).

While many of the studies we cite above report increasing wage inequality in India since 1991, this result seems to crucially depend on how wage inequality is measured and which data is used. Many studies confirm an empirical link between rising inequality and the 1990s economic liberalisation, with trade-induced SBTC probably the best supported argument to date. We add to the existing literature by exploring a novel data source to calculate skill premia and, in particular, by constructing a synthetic control group to investigate in detail the development of these skill premia in the post-liberalisation period.

3.3 Data Description

3.3.1 UBS Prices and Earnings Survey

We calculate skill premia using salary and wage data from the UBS Prices and Earnings survey. This data is described shortly in section 1.5 and in detail in appendix A. This survey has been conducted every three years since 1970, producing a dataset that displays high consistency over time and good global comparability, as in each country the survey was conducted contemporaneously with an identical questionnaire and comparable methods. Of the 35 cities that appeared in every, or nearly every wave, 17 are in Europe, six in Latin America, five in North America, four in Asia, one in Africa, one in the Middle East, and one in Australia.[4] Data for Mumbai are available from 1982 onwards every three years, so the pre-treatment data points are 1982, 1985, 1988 and 1991. All cities other than Mumbai with data available for that period are considered for inclusion in the synthetic control group.

We use the survey measure "gross annual income" in constructing the skill premia.[5] The data are provided in US dollars (USD) as converted from local currency using the average market exchange rate at the time of the survey. The skill premium for each city was calculated as the ratio of the skilled to unskilled wage index, with each index representing the average of wages for skilled or unskilled professions. Clearly, the actual level of skill premium is related to the selection of professions in the UBS survey, and hence it is not directly comparable to other skill premium calculations. The advantage of our approach is that the wage indices and skill premia are constructed identically between all cities and thus are perfectly comparable over time and geography, making them a suitable device for use in a synthetic control group.

As in section 1.5 we assign a skill level to each profession based on the following breakdown:

– **Skill level 1:** Obligatory schooling only; largely unskilled labour; very limited training.

– **Skill level 2:** Obligatory schooling plus full apprenticeship or extensive practical training. Or completed high school and some practical training.

– **Skill level 3:** Completed high school and university or college education.

Skill level 1 professions therefore include bus drivers, factory or textile workers, saleswomen, and construction workers. Skill level 2 professions include secretaries, auto mechanics, bank tellers or credit clerks, cooks and skilled industrial workers. Finally, skill level 3 professions include primary school teachers, electrical engineers and department managers in industry.

3.3.2 Data Strengths and Limitations

A key strength of the UBS Prices and Earnings survey is the identical definition of workers and professions across cities, enabling the construction of a consistent and comparable outcome measure, i.e., the skill premia, an essential ingredient of the synthetic control

[4] See table A.1 in the appendix for information on the survey periods, and table A.3 for the cities included.

[5] This measure is defined in the survey as "Gross annual income (sum of hourly, weekly or monthly earnings) taking into account family status and tax allowances including all fringe benefits such as profit participation, bonuses, vacation money, additional monthly salaries as bonus payments, allowances for children etc. but excluding overtime compensation". The complete questionnaire is available upon request.

method. We consider this a significant advantage compared to using nearly any alternative data source with a comparably long history: Most wage data are compiled from a variety of sources including each country's statistical offices, tax offices, labour unions, and so on. Wage indices constructed from such data, for example for the services or manufacturing industries, will frequently reflect different professions as not all of them exist in all countries, or suffer from differing index compositions of professions between countries. Additionally, the skill premium is frequently defined as a sector-specific premium, e.g., services to manufacturing wages. This is mostly done due to data constraints as less aggregated wage data is often not available. The data we use enables us to compare wages of a smaller number of professions, which, however, exist and are defined in the same way across all cities considered.

Focusing on a group of selected large cities has the advantage that we do not have to worry about urban/rural compositions, i.e., we do not compare, for example, the wage of a bus driver in a rural environment in one country to that of a bus driver in an urban environment in another country – the "large city" context is always the same for all data points. While many analyses use country data rather than cities, for our purpose city data seems sufficiently appropriate, as economic reforms would likely impact rural and urban earnings very differently, and changes in skill premia would then depend on the composition of each local sub-economy. Thus, when using a synthetic control method, it seems a valid approach to use city data to make use of the similarities of the economic structures. It should also be noted that this approach is not entirely different from other studies because many databases used for country comparisons must often make do with data primarily from urban areas – less affluent regions and rural areas are typically underrepresented; see for example O'Connor (2008, p. 3) and Van Ark and Monnikhof (2000, p. 6).

In terms of limitations, the data do not reflect differences in the structure of labour markets, as for example household survey data would. This wage data therefore cannot be assumed to represent all professions within a city, but only certain professions that exist in all cities. For our current application this limitation does not seem very serious as the professions for which data is provided represent a good cross section in both manufacturing and services, and differing skill levels. A more serious limitation is the low data frequency. Clearly, our data will not capture the very short-term variations in wages. This is a disadvantage and an annual or bi-annual frequency would have been preferred. However, as the effects of India's economic liberalisation can be expected to dissipate slowly through the economy a focus on the medium- and long-term seems acceptable. Additionally, the synthetic control group methodology is well suited to small sample sizes and is not significantly limited by the low data frequency. Finally, despite the identical and precise definitions of the professions in the survey, it is still possible that in different countries somewhat different skills are needed for the same professions. Unfortunately, this limitation cannot be excluded.

3.3.3 Descriptive Statistics

This section provides some descriptive statistics and graphical representations of the macro-economic context and the UBS Prices and Earnings wage and skill premium data used in this study.

Macro-economic context

Figure 3.1 shows real GDP growth rates in India in the decades before and after the 1991 economic liberalisation. We use local polynomial regression fitting to add a smoothed function and standard error. Average real GDP growth in India was about 5.1% between 1980 and 1991, about 6.1% between 1992 and 1999, and about 6.9% between 2000 and 2009, according to IMF data. This accelerating trend in growth is confirmed when adjusting for population growth: GDP per capita increased by an average of only around 1.2% between 1960 and 1980, 2.8% in the 1980s, 4.1% in the 1990s and 6.2% in the 2000s (see Mehta and Hasan, 2012), which points at an accelerating increase in average well-being. However, this does not yet explain how this increase in wealth was distributed, a question which lies at the core of the current study.

Figures 3.2 and 3.3 show import and export volume growth for India over the same period, again with a smoothed function included. We see that export growth already started to accelerate in the mid-1980s, reaching its peak at over 20% year-on-year in 2004, but falling back sharply during the global financial crisis in 2008 and 2009. Import volumes fell sharply during India's balance of payments crisis in 1991 and 1992, in line with the dramatic devaluation of the Indian rupee. However, import growth recovered strongly and accelerated after 1991 averaging 12.1% between 1992 and 1999 and 10.7% between 2000 and 2009, according to IMF data. Clearly, increased trade in post-1991 India was a cornerstone of its subsequent economic success.

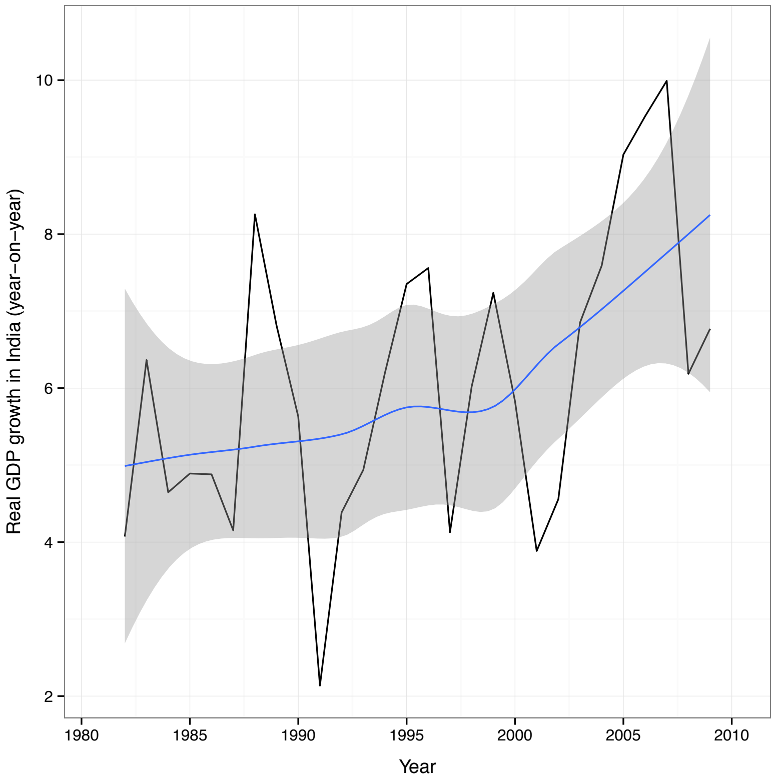


Figure 3.1: India real GDP growth. Data source: IMF.

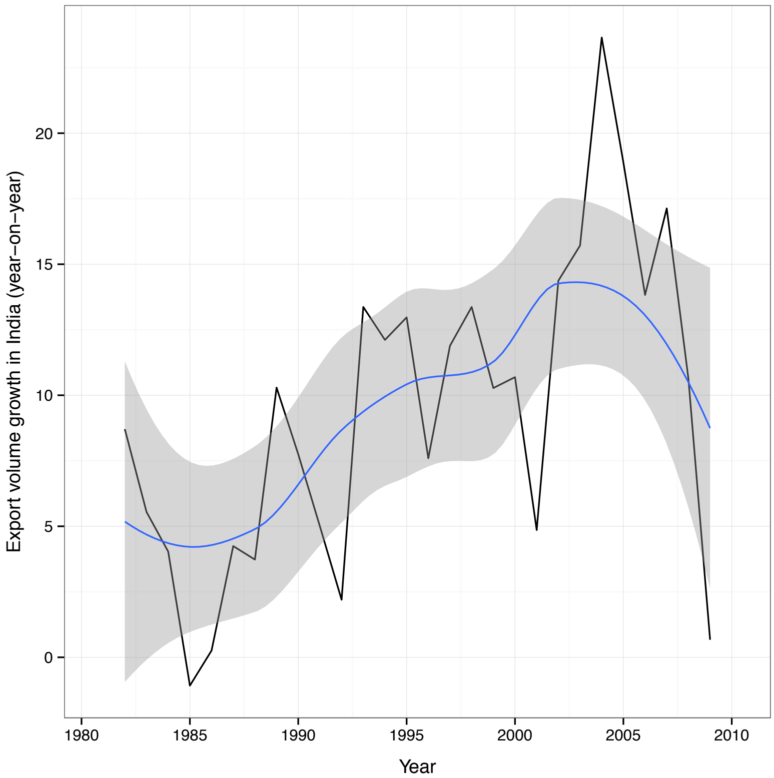


Figure 3.2: India export volume growth. Data source: IMF.

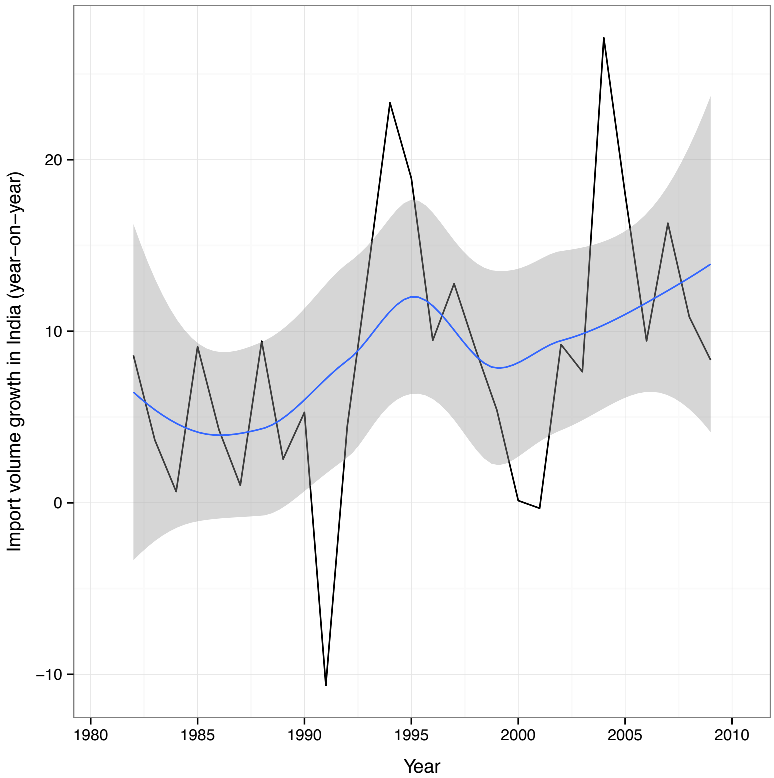


Figure 3.3: India import volume growth. Data source: IMF.

Wage data from the UBS Prices and Earnings surveys

Figures 3.4 to 3.6 show the average wages in nominal USD for Mumbai, Bogota, Jakarta, London, Oslo and Seoul for each of the three skill levels. The local currency wages were converted to USD using the average spot exchange rate during each survey period. These cities were selected to show examples of high, medium and low wage levels in the dataset. Measured in USD, average wages in Oslo were highest for this selection of cities for all three skill levels, followed by London's wages. They are lowest for Jakarta and Mumbai, which show relatively similar wages for each skill level. Wages for unskilled workers (level 1, or L1) in Bogota are similar to those in Jakarta and Mumbai, but for skill levels 2 (L2) and 3 (L3), wages are more favourable in Bogota. Wages in Seoul take up the middle ground. The rising nominal wages in USD for Oslo, London and Seoul reflect that in USD terms these wages experienced some form of inflation adjustment. The rather lower and more stable levels for Mumbai, Jakarta and Bogota reflect that in USD terms these wages lost purchasing power — in part attributable to the local currency devaluation versus the USD.

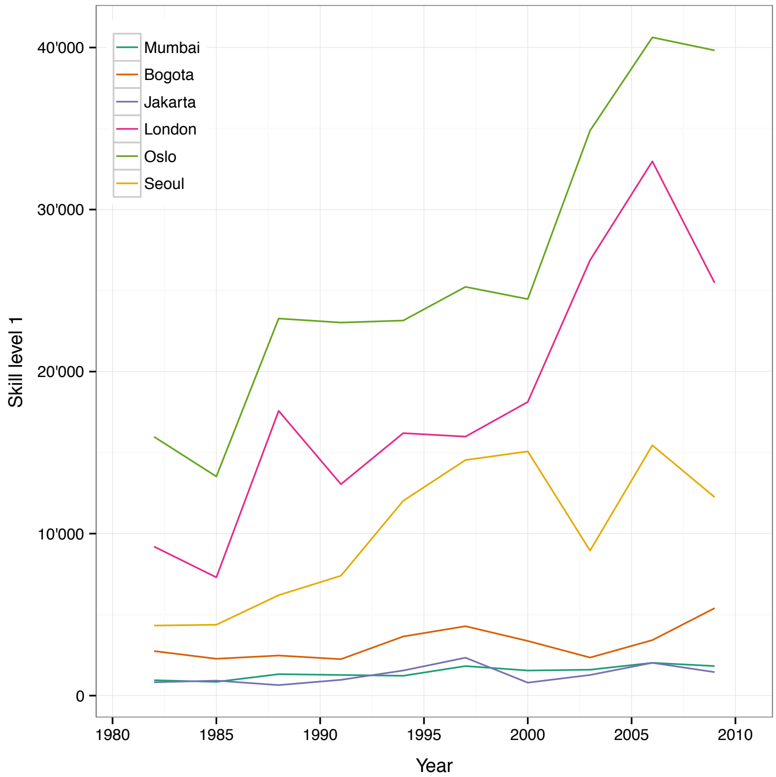


Figure 3.4: Average skill level 1 wage for Mumbai and five selected control group cities (nominal USD)

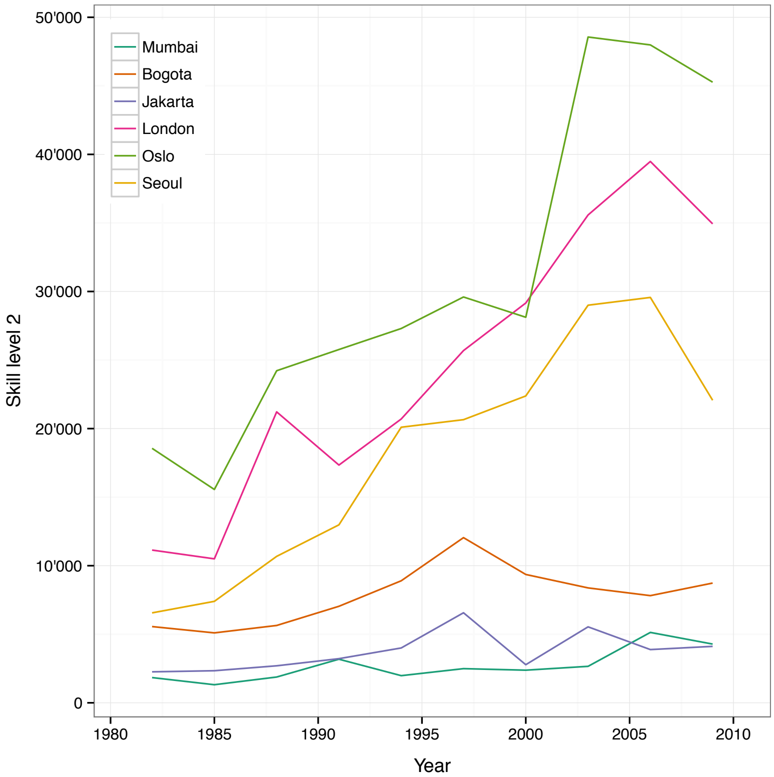


Figure 3.5: Average skill level 2 wage for Mumbai and five selected control group cities (nominal USD)

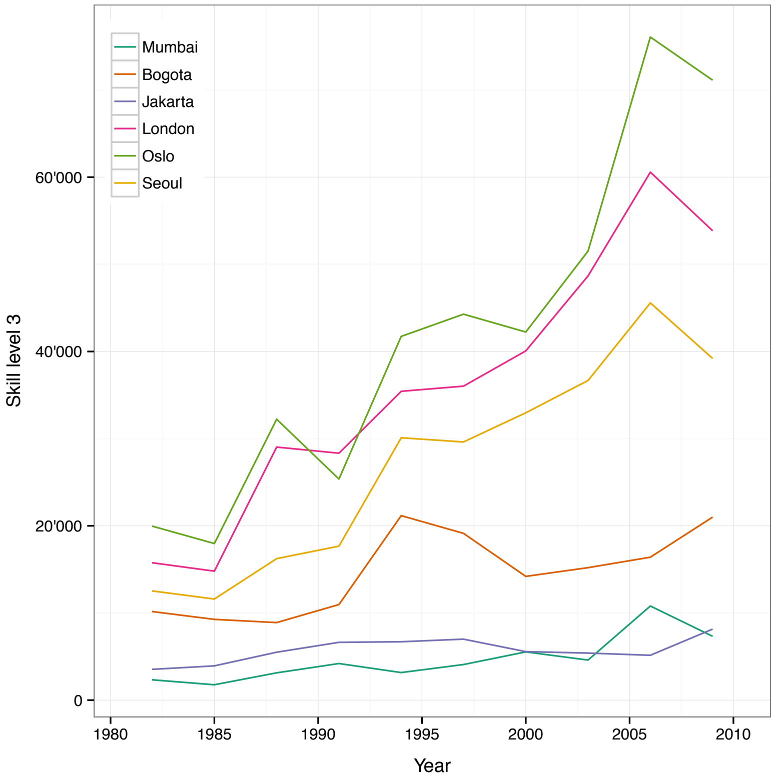


Figure 3.6: Average skill level 3 wage for Mumbai and five selected control group cities (nominal USD)

Skill premia from the UBS Prices and Earnings survey

Figures 3.7 to 3.9 show the skill premium ratios L3/L1, L3/L2 and L2/L1 for the same selection of cities. The use of ratios means that currency and inflation effects are eliminated. On average, countries with a higher GDP-per-capita tend to have lower and less volatile skill premia in this dataset, as represented here by Oslo and London. The higher the skill premium levels, the more volatile the series tend to be, as seen here for Jakarta and Bogota. Out of the 35 cities in the dataset, Mumbai's skill premium ratios are in the third highest quartile during most survey periods, but lower than the average for the emerging markets.

As could be expected for the dataset as a whole, the skill premium ratio L3/L1 is on average highest, followed by L2/L1, with L3/L2 lowest.[6] A first look at the data thus points to highest relative return increases for workers when moving from unskilled to medium-skilled jobs.

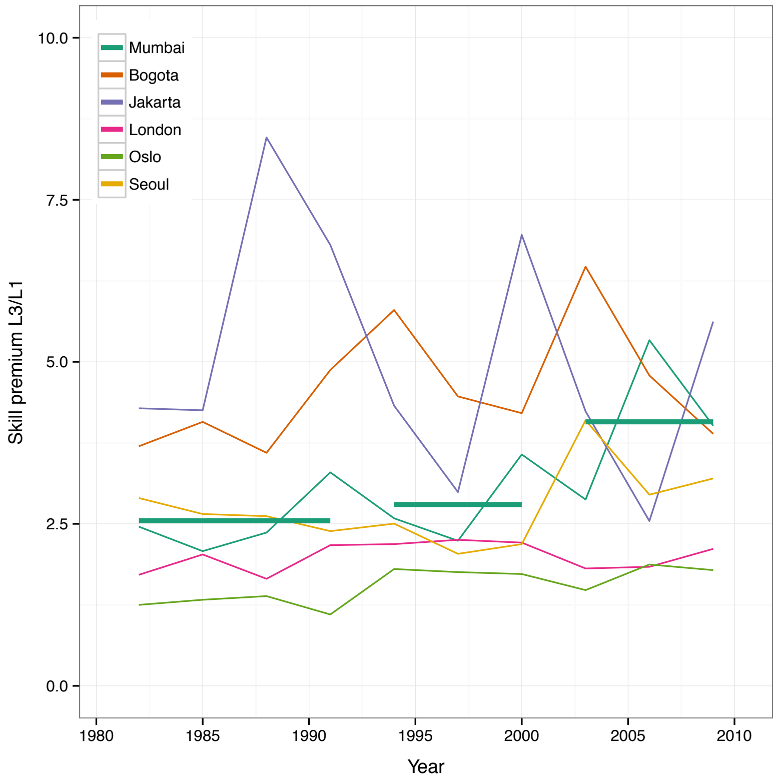


Figure 3.7: Skill premium ratio L3/L1 for Mumbai and five selected control group cities

[6]This can be easily seen when comparing Figures 3.7 to 3.9. Note the difference in scale for the ordinate of Figure 3.7.

The green horizontal lines represent Mumbai's average skill premium for the periods 1982-1991, 1994-2000 and 2003-2009. For the skill ratio between highly skilled and unskilled workers L3/L1 we see that the premium rises moderately at first from an average of 2.6 in 1982-1991 to 2.8 in 1994-2000, but then jumps strongly to an average of 4.1 in 2003-2009. Over the complete period this indicates substantial losses of low-skilled versus high-skilled workers' relative wages and points to an increase in wage inequality.

In Figure 3.8 we see that the situation for medium versus low-skilled workers' wages in Mumbai was substantially different. In the initial post-liberalisation years the average L2/L1 ratio falls from 1.9 to 1.5. However, medium-skilled workers' relative wages recovered strongly in the 2000s with the L2/L1 skill ratio reaching a 2003-2009 average of 2.2, higher than the pre-liberalisation levels. Thus, while wage inequality between medium and low-skilled workers fell initially, over the complete period L2/L1 wage inequality as measured here increased nonetheless.

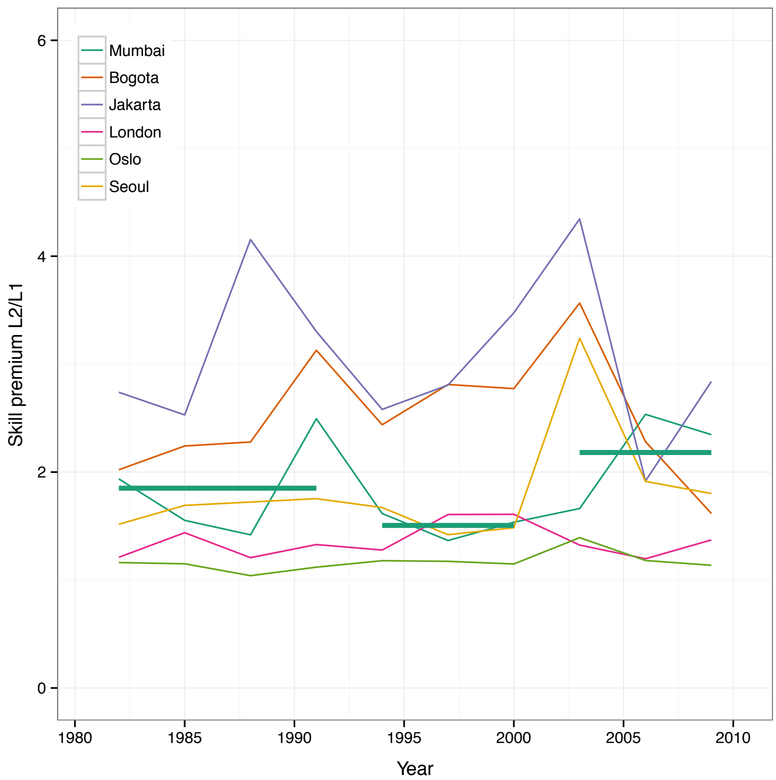


Figure 3.8: Skill premium ratio L2/L1 for Mumbai and five selected control group cities

The skill premium earned by highly qualified workers as compared to medium skilled workers rises from an average of 1.4 in 1982-1991 to a stable level of 1.8-1.9 for 1994-2009.

Thus, medium skilled workers' relative wages in Mumbai fell as compared to highly skilled workers in the initial post-liberalisation period. However, this trend did not continue and medium and highly skilled workers' wages on average increased at the same pace in the later post-liberalisation period. Over the complete period, however, this L3/L2 wage inequality also increased as measured by this data.

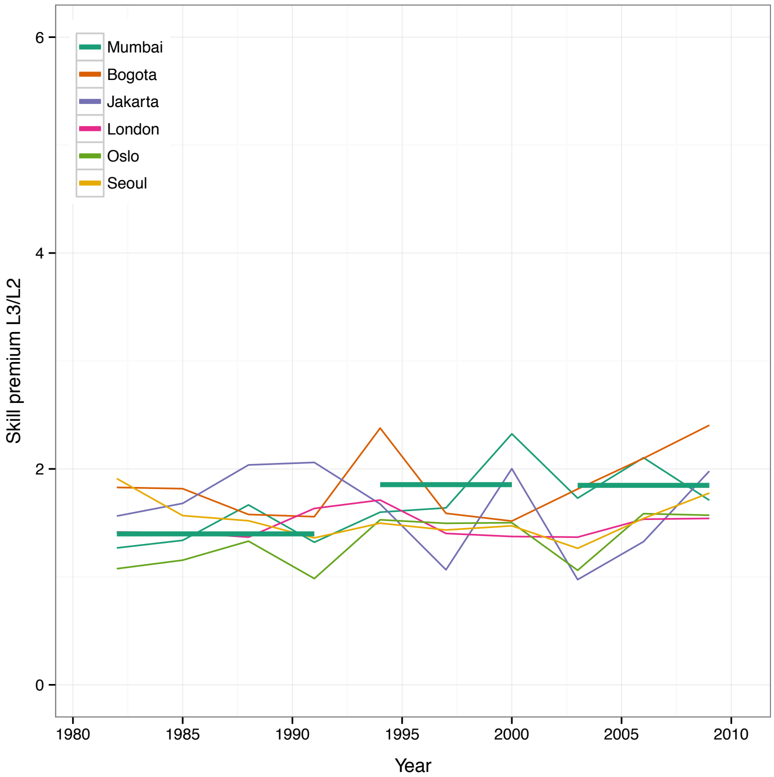


Figure 3.9: Skill premium ratio L3/L2 for Mumbai and five selected control group cities

3.4 Synthetic Control Group Methodology

3.4.1 Determining a Suitable Comparison Group

While we can measure and display India's skill premia as in figures 3.4 to 3.9, we cannot conclude from the observed trends that these were driven by India's dramatic economic and trade liberalisation, or whether they were just the result of common globalization trends. More technically, we are faced with the problem of the unobserved counterfactual, i.e., the development of skill premia in India if no economic reforms had taken place are unobserved. In order to address this problem and to estimate the magnitude of the effect of India's economic reforms on wage inequality we construct a synthetic control group. Abadie and Gardeazabal (2003) and Abadie et al. (2010) suggest a data-driven procedure for control group selection which produces a weighted average of control group units that approximate the most important characteristics of the treated unit during the pre-treatment period. The composition of the control group is transparent in that the contribution of each available control unit, and the similarity of the synthetic control group to the treated unit before treatment (or lack thereof) is made explicit (Abadie et al., 2011, p. 2). In contrast to the commonly applied difference-in-differences model, the key advantage of this approach is that it allows for the effect of unobserved characteristics not fixed over time.[7]

3.4.2 Estimation

In order to construct a suitable control group for Mumbai, weights need to be determined such that the synthetic control group "resembles the treated unit in all relevant pre-intervention characteristics" (Abadie et al., 2011, p. 3), i.e., the synthetic control should be as similar as possible to Mumbai in terms of the outcome variable during the pre-liberalisation period. We use the average skill premium and the average skill premium growth rates from 1982-1991, and the absolute skill premium levels in 1988 and 1991 as matching factors. We additionally allow for the following pre-treatment measures as potential predictors of the skill premium:[8]

1. Mean real GDP growth rates
2. Nominal GDP per capita in USD
3. Nominal PPP-adjusted GDP per capita in USD

[7] For a formal and more detailed discussion of the synthetic control group methodology see Abadie et al. (2010), Abadie et al. (2011), and Mukherji and Mukhopadhyay (2011).

[8] Data for the predictors 1-6 sourced from the IMF World Economic Outlook Database, September 2011 edition.

4. Export volume, percentage change from year ago
5. Import volume, percentage change from year ago
6. Population growth
7. Education index, reflecting overall levels of education[9]
8. Expected years of schooling of children, in years[10]
9. Mean years of schooling of adults, in years[11]
10. Human Development Index (HDI)[12]

Estimation is performed using the statistical programming tool R and the package *Synth*,[13] described in Abadie et al. (2011). Control groups are estimated separately for each of the three skill premium ratios L3/L2, L3/L2 and L2/L1. The different skill premium levels 1, 2 and 3 reflect different sectors and therefore it can be expected that they are affected differently (and in a non-linear fashion) by liberalisation. This warrants allowing for different synthetic control groups for each skill premium ratio. The optimisation results and the paths comparing Mumbai's skill premium development to the path of the counterfactual are shown below. During the optimisation process several of the potential predictors were found not to improve the pre-treatment fit and were thus omitted from the subsequent analysis. These include the nominal GDP and PPP-adjusted GDP per capita, the education index, the expected years of schooling of children, the mean years of schooling of adults, and the HDI.

3.5 Results

3.5.1 Skill Premium for High-Skilled versus Low-Skilled Professions L3/L1

For the L3/L1 ratio, the synthetic control group is made up of Oslo (0.332), Helsinki (0.223), Buenos Aires (0.191), Seoul (0.117), London (0.087), Jakarta (0.035), Sao Paulo (0.011) and Luxembourg (0.004), with the figures in brackets indicating the city weights. Just over half of the total weighting stems from cities in developed markets, the remaining share from cities in emerging markets. This can be explained by the fact that in the 1980s and early 1990s Mumbai's skill ratio as calculated from the UBS Prices and Earnings

[9]Source: HDRO calculations, as of 2011.
[10]Source: UNESCO, as of 2011.
[11]Source: HDRO updates of Barro and Lee (2010), UNESCO Institute for Statistics, as of 2011.
[12]Source: HDRO calculations based on data as of 2011 from UNDESA, Barro and Lee (2010), UNESCO Institute for Statistics, World Bank and IMF.
[13]Sourced from R Core Team (2011-2013).

data were on the lower end compared to most emerging markets, but higher than most developed markets. Thus an optimal control group includes a significant share of developed market donors.

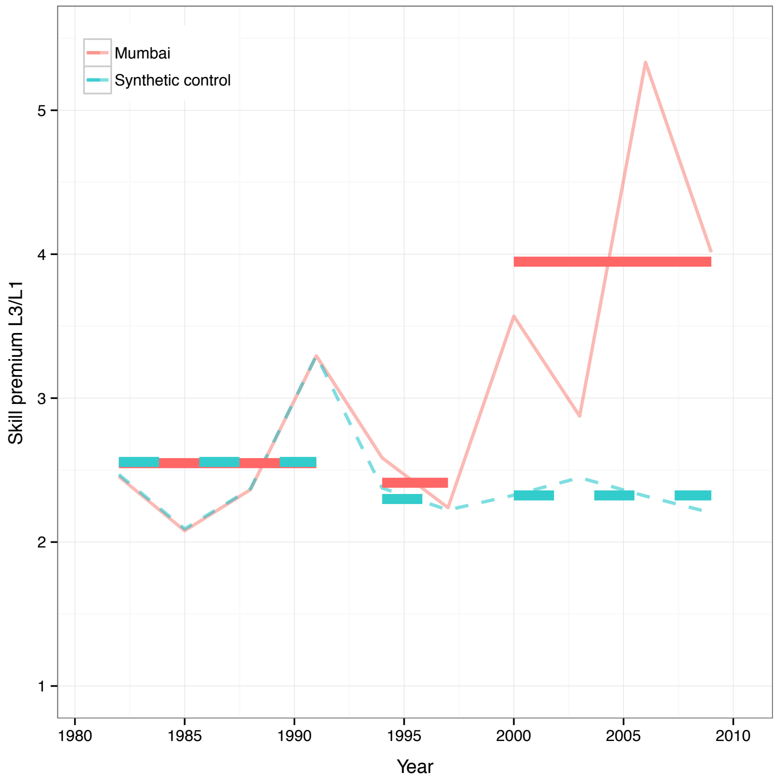


Figure 3.10: Skill premium path plots showing period means for Mumbai and its synthetic control for skill ratio L3/L1

Figure 3.10 shows the path of Mumbai's L3/L1 skill premium compared to its synthetic control. For the pre-treatment period 1982 to 1991, the synthetic control group achieves a near-perfect fit. The paths of the L3/L1 skill premium in Mumbai and the synthetic control also stay very close to each other during the 1990s. Only after 1997 and especially in the 2000s the L3/L1 skill premium in Mumbai rises significantly above the synthetic control. Therefore, a first conclusion is that during the first phase of liberalisation the relative effects on the high-skilled professions (L3) and low-skilled professions (L1) are not distinguishable from what would be expected without the liberalisation. During the following stages, however, high-skilled workers started to benefit relatively more, and this benefit would not have been expected without the liberalisation (as from 1994 onwards the skill ratio of the synthetic control group stayed at a stable level of around 2.3). Therefore, the rise in inequality between high- and low-skilled wages from the late 1990s onwards can likely be attributed to the reform process in India.

Figure 3.10 includes averages of the L3/L1 skill premium for Mumbai and its synthetic control for the pre-treatment period 1982-1991, and for the early and later post-treatment periods 1994-1997 and 2000-2009. While the means are identical in the pre-treatment period and very similar still during the mid 1990s, the averages after 2000 differ sharply with Mumbai's skill premium at an average of 3.9, while the synthetic controls average was 2.3.

3.5.2 Skill Premium for High-Skilled versus Medium-Skilled Professions L3/L2

For the L3/L2 ratio, the synthetic control for Mumbai consists of Oslo (0.503), Jakarta (0.261), Stockholm (0.202) and Brussels (0.034). Figure 3.11 displays the paths of the L3/L2 skill premium in Mumbai compared to its synthetic control. Again, there is an almost perfect fit of the synthetic control during the pre-treatment period.[14] From the mid 1990s, Mumbai's skill premium starts to deviate upwards relative to the synthetic control, pointing to an increase in relative wages for the high-skilled relative to the medium-skilled professions. As the skill premium L3/L2 of the synthetic control rebounds after 2003 and starts to fall back for Mumbai, this relative advantage disappears for the last data point in 2009. Overall, the L3/L2 skill premium level as calculated from the UBS Prices and Earnings data suggests that there has been a substantial rise in L3/L2 wage inequality that can be attributed to India's economic liberalisation until 2006, with a late relative catch-up of medium-skilled wages.

Figure 3.11 includes the means for Mumbai's L3/L2 skill premium and its synthetic control for the pre-treatment period 1982-1991, and for the early and later post-treatment periods 1994-1997 and 2000-2009. While these decade means are nearly identical in the pre-treatment period, the averages increasingly differ in the post-treatment periods.

3.5.3 Skill Premium for Medium-Skilled versus Low-Skilled Professions L2/L1

The synthetic control group for the L2/L1 skill premium is made up of two cities, Madrid (0.579) and Bogota (0.421). As before about half of the weighting stems from a developed and half from an emerging market city. Figure 3.12 shows the path of Mumbai's L2/L1

[14] The export volumes predictor variable was removed as its inclusion resulted in the optimisation algorithm not producing a computational result.

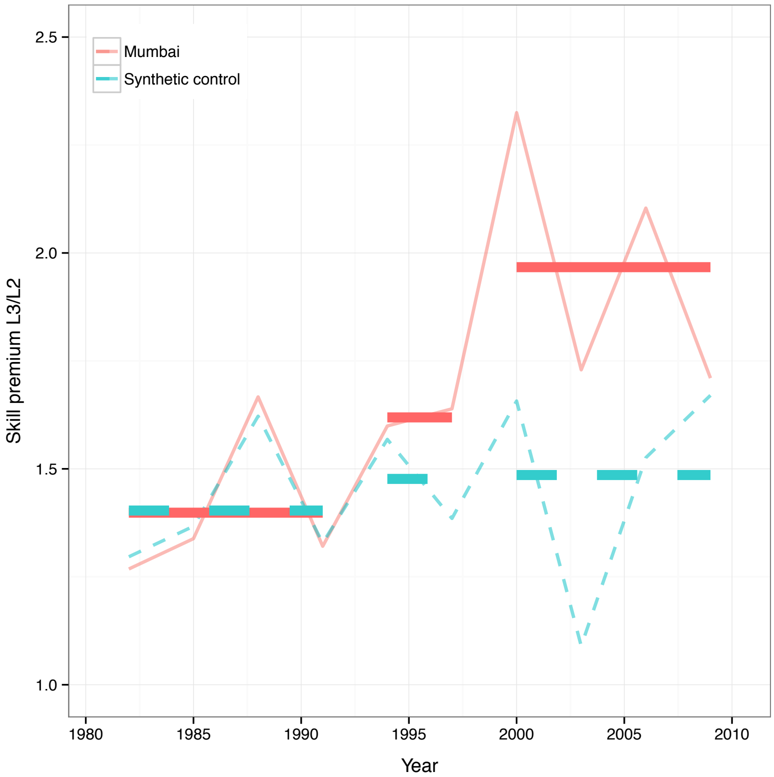


Figure 3.11: Skill premium path plots showing period means for Mumbai and its synthetic control for skill ratio L3/L2

skill premium, compared to its synthetic control. In contrast to the previous two skill premium ratios, the synthetic control achieves only a moderate fit for the pre-treatment period, and hence, the donor cities do not provide an ideal pool for the control group.[15] We observe diverging trends from 1991 onwards with Mumbai's L2/L1 skill premium falling below its synthetic control between 1994 and 2000, but rising above it from 2003 onwards. This would indicate that wages in medium-skilled professions (L2) fell relatively to wages in low-skilled professions (L1) in the first phase of India's economic liberalisation, and that this fall was sharper than the synthetic control suggests it would have been if no liberalisation had taken place.

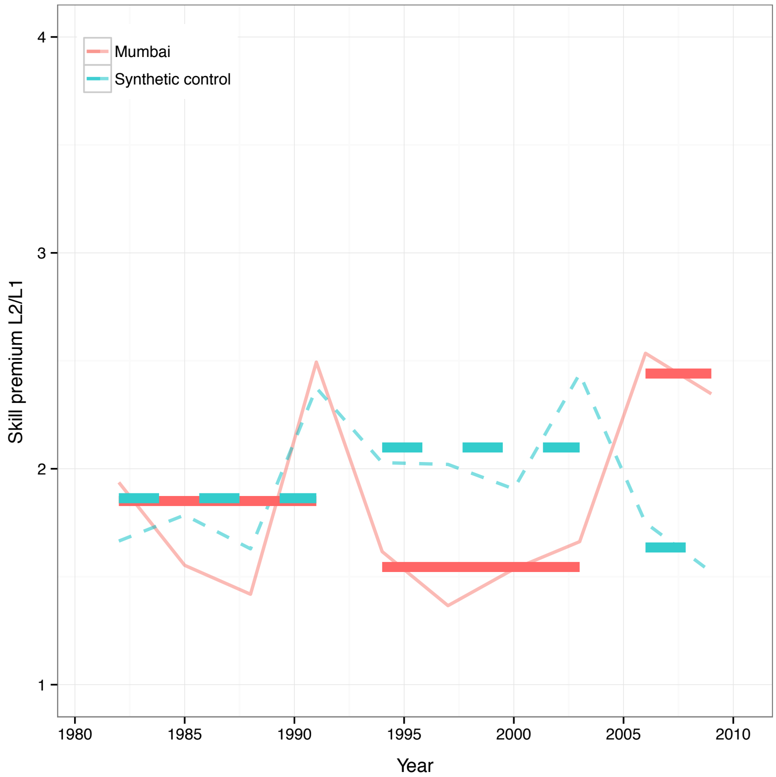


Figure 3.12: Skill premium path plots for Mumbai and its synthetic control for skill ratio L2/L1

After 2003, this effect reversed. A possible explanation is that the initial steps of liberalisation had a different impact on wages than the less dramatic, but ongoing opening of the Indian economy. One can expect that the different factors influencing the skill premia

[15] Two predictor variables (population growth and 1988 skill premium level) were removed as their inclusion worsened the pre-treatment fit.

might take effect at different phases during the economic opening process, in particular as growth rates in India accelerated substantially in the later post-treatment period. For example, mechanisms such as SBTC could be expected to impact the skill premium rather more gradually than the more immediate effects of tariff reductions.

3.5.4 Predictors and Predictor Weights

Table 3.1 provides an overview of the characteristics of Mumbai's skill premium and its predictors (as described above), and the fit of the predictors of the synthetic control group as compared to the sample mean for the donor pool. In the optimisation process these characteristics determine the weights of the predictors, which are shown in the last column.

Table 3.1: Mumbai and donor pool predictors

	Treated	Synthetic	Donors	SE Donors	Weights
A. L3/L1 skill premium					
Real GDP growth	5.184	3.166	3.090	0.316	0.006
Export volume growth	4.847	4.935	5.918	0.459	0.110
Import volume growth	3.388	4.036	5.411	0.551	0.027
Population growth	2.166	0.767	0.960	0.142	0.000
L3/L1 skill premium mean	2.548	2.555	2.762	0.204	0.513
L3/L1 skill premium growth	12.566	8.730	3.532	1.989	0.010
L3/L1 skill premium 1991	3.294	3.295	2.822	0.261	0.247
L3/L1 skill premium 1988	2.365	2.369	2.635	0.212	0.087
B. L3/L2 skill premium					
Real GDP growth	5.184	3.265	3.090	0.316	0.001
Import volume growth	3.388	3.364	5.411	0.551	0.130
Population growth	2.166	0.714	0.960	0.142	0.000
L3/L2 skill premium mean	1.398	1.416	1.689	0.053	0.328
L3/L2 skill premium growth	3.105	1.287	2.563	1.920	0.079
L3/L2 skill premium 1988	1.667	1.639	1.650	0.053	0.208
L3/L2 skill premium 1991	1.321	1.337	1.722	0.086	0.254
C. L2/L1 skill premium					
Real GDP growth	5.184	3.340	3.090	0.316	0.000
Export volume growth	4.847	6.915	5.918	0.459	0.019
Import volume growth	3.388	6.273	5.411	0.551	0.000
L2/L1 skill premium mean	1.851	1.885	1.621	0.097	0.641
L2/L1 skill premium growth	15.776	14.920	1.799	1.408	0.050
L2/L1 skill premium 1991	2.494	2.403	1.614	0.108	0.290

Notes: Columns display the values for the treated city (Mumbai), the mean values for the synthetic control (as described in the text), the mean values for the complete donor pool, the standard error of the mean for the complete donor pool, and the weights assigned for the matching.

For L3/L1 and L3/L2 an excellent pre-treatment fit is achieved, for L2/L1 the fit is moderate. Overall this indicates that Mumbai's data does not lie outside the convex hull of the donor data. Running the equivalent optimisation allowing only for cities in emerging markets in the donor pool provides a significantly poorer fit (results not reported here). In terms of predictor weights, the linear combinations of the pre-treatment skill premia receive the highest weightings. For all three skill ratios the mean of the specific ratio over the pre-treatment period receives the highest weighting and the 1991 ratio level the second highest. For the other predictors no single one systematically receives a stronger weight.

3.5.5 Robustness Checks

To better understand the size and relevance of the deviations between the skill ratio trends in Mumbai and its synthetic control group, we employ a series of placebo tests. These can take on many different forms, for example, by reassigning the treatment to cities in which no such treatment actually occurred (see Abadie et al., 2011, p. 14). We treat the other cities as equivalent to Mumbai assuming treatment also to start in 1991. As recommended in Abadie et al. (2010) and as implemented in Abadie et al. (2011) we exclude cities with a poor fit for the pre-treatment period. This poor fit is usually due to the city's data lying close to the edge or outside the convex hull of the donor cities, i.e., having unusual pre-treatment characteristics. These cities can be excluded without weakening the results of the placebo study.

In figures 3.13 to 3.15 we plot the gaps of the placebo tests run on all other cities for which the data is available and for which the optimisation process has a computational result. The grey lines represent the gap between the (hypothetical) treatment and its synthetic control outcome for each donor city. The black line represents the equivalent gap for Mumbai. As seen in figure 3.13 for L3/L1, only two out of 19 placebo tests (10.5%) – namely those for Johannesburg and Mexico City – show gaps that are larger than Mumbai's L3/L1 gaps at their respective absolute maxima. Singapore and Hong Kong's gaps are also large, but only comparable to Mumbai's gaps in the early 2000s, not the larger gaps in the late 2000s. This indicates that the relative decline in low-skilled versus high-skilled wages and thus the rise in wage inequality in Mumbai from 1997 onwards is not only larger than the synthetic control group would suggest, but also that this difference is probably larger than would be expected if the economic and trade liberalisation had not occurred.

For L3/L2 in figure 3.14, only Johannesburg shows up a larger absolute maximum gap out of the 22 placebo comparisons (4.5%). Thus, not only does the rise in the L3/L2 skill premium stand in contrast to the fall in skill premium of the synthetic control. The gap between the two, in particular between 2000 and 2006, is also larger than the placebo tests suggest would be the case if this was driven by chance, not by an actual treatment. By the end of the post-treatment period the gap narrows to very low levels in the range of the

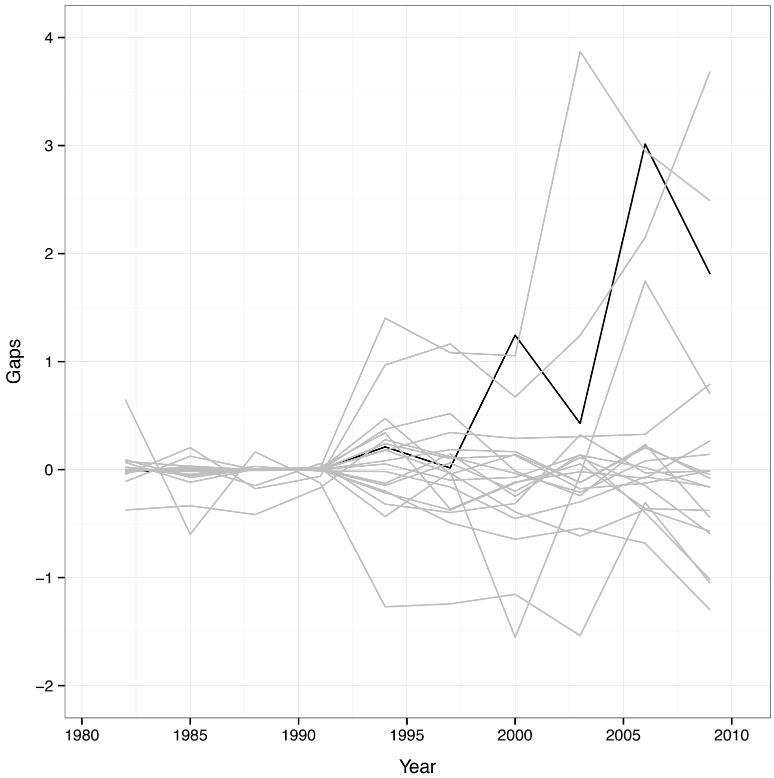


Figure 3.13: Placebo test for the L3/L1 skill ratio

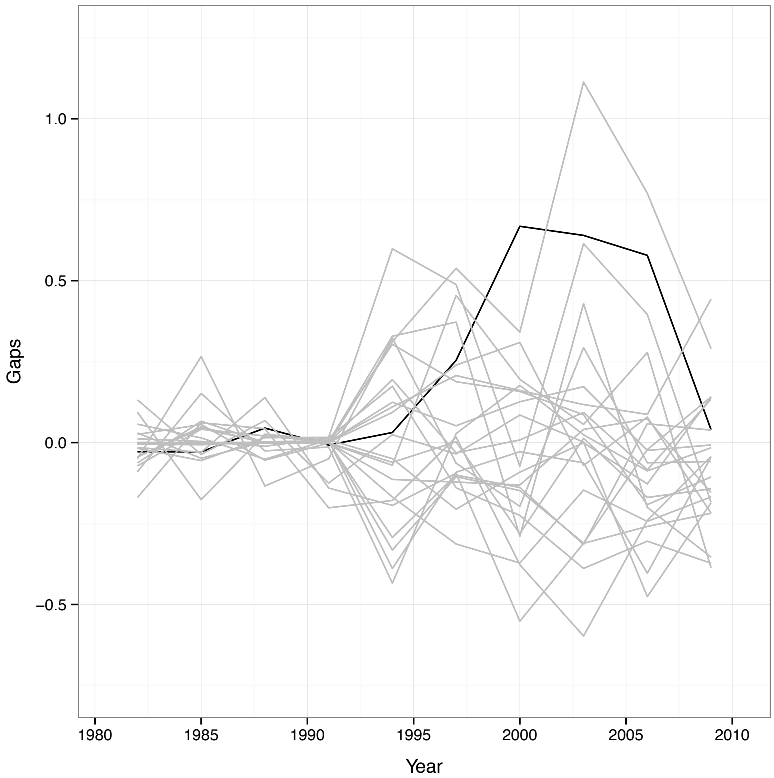


Figure 3.14: Placebo test for the L3/L2 skill ratio

average placebo results.

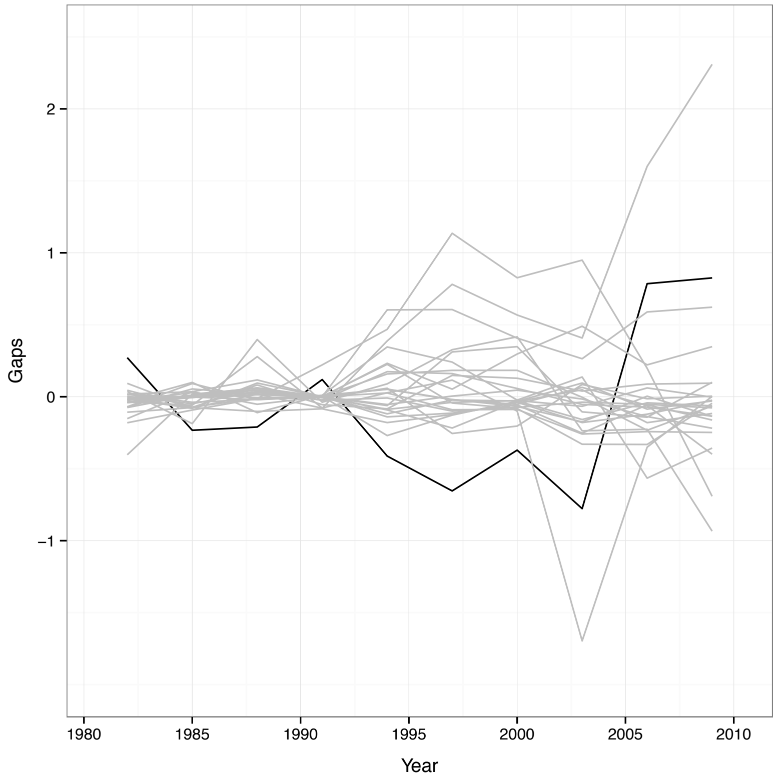


Figure 3.15: Placebo test for the L2/L1 skill ratio

For L2/L1 in figure 3.15, four out of 24 placebo tests (16.6%) show gaps that are larger (in absolute terms) than Mumbai's gap at the respective extrema (Mexico City, Singapore, Bogota and Manila). Thus, for L2/L1 too the placebo tests confirm that the estimated gap for Mumbai is unusually large relative to the distribution of gaps for the donor pool cities. This indicates that the relative decline in medium-skilled professions' wages to unskilled professions' in the first phase of India's economic liberalisation was sharper than the synthetic control would suggest and probably larger than would be expected if no treatment had taken place. The same is true for the relative rise in medium-skilled wages versus low-skilled wages after 2003.

While the placebo tests suggest that the gaps in the skill premia between Mumbai and its synthetic control are unusually large, these results are not fully conclusive, as a few donor cities do show up similarly large gaps. Since the pre-and post-treatment periods span over 25 years, we cannot exclude that individual cities in the donor pool have experienced similarly dramatic changes comparable to India's and that these had an impact on their skill premia, too. We therefore conclude that given fewer than one sixth of the placebo runs produce larger gaps than Mumbai our effects of liberalisation on wage inequality are rather unlikely to have been produced by chance.

3.5.6 Alternative Approach: Difference-in-Differences

As a second robustness check, we estimate the effect of economic liberalisation on wage inequality using a standard difference-in-differences (DID) approach. DID is based on the idea of computing two differences, one over time (before-after) and one across subjects (treated-untreated). By subtracting the pre-treatment mean difference of the outcome between the treated and the untreated group from the post-treatment difference, time-invariant factors specific to only the treated or untreated units are accounted for, eliminating the type of selection bias related to these time-invariant characteristics. However, the DID method cannot account for time-variant factors, and thus the critical identifying assumption is that the counterfactual trend in the treated unit is equivalent to the trend in the control group.

The main differences between the synthetic control group methodology and DID are threefold: First, the former uses a data-driven process to create a control group which approximates the most important characteristics of the treated unit during the pre-treatment period. The composition of this synthetic control is transparent in that the contribution of each available control unit and the similarity (or lack thereof) of the synthetic control group to the treated unit before treatment is made explicit (Abadie et al., 2011, p. 2). The latter permits the researcher to select a control group according to what plausibly seems sensible (and, of course, what data is available). Second, the former allows for the effect of potentially unobserved characteristics that are not fixed over time, while the latter corrects only for time-invariant characteristics. Third, DID generally uses two distinct points in time on which the comparison is based, usually the period before the treatment occurs, and a period after the treatment. If more pre- or post-treatment data is available, this is averaged to one single pre-treatment and one single post-treatment value. The synthetic control group is aimed at observing the effects as they unfold over several time periods.

As the selection of a control group for DID estimation can be done in many ways, we here select examples of control groups that seem plausible and enable an interesting comparison to the previous methodology: First, we use the synthetic control group that was calculated previously, but now use DID methodology. Second, we use the average over all donor cities available as our DID control group.

A simple DID can be calculated by differencing the double differences, or equivalently as we do it here, by finding the DID effect by estimating the following equation with OLS:

$$Y_{i,t} = \alpha + \beta T_i + \gamma P_t + \delta(T_i \cdot P_t) + \epsilon_{i,t} \tag{3.1}$$

$Y_{i,t}$ is the skill premium in City i and Year t.
T_i is the binary variable (dummy) for treatment, = 1 if City i is Mumbai, else = 0.
P_t is the binary variable (dummy) for the period, = 0 for pre-treatment 1982-1991, = 1 for post-treatment.
$T_i \cdot P_t$ is the interaction term.
α, β, γ and δ are regression variables to be estimated: α (intercept) is the average skill premium for the untreated control group in the pre-treatment period. β is the initial difference (i.e. difference of pre-treatment averages) between treated and untreated units, while γ is the difference of pre- and post-treatment averages for the untreated control group. Finally, the coefficient of the interaction term, δ, is the DID effect that we are interested in. It indicates the treatment effect, i.e. the effect that cannot be explained by the control group.

Difference-in-Differences results for L3/L1

Using the synthetic control group calculated previously as the untreated group, and taking the data over the complete sample period 1982-2009, the DID estimate for the treatment effect is 1.137. This can be calculated as follows (see table 3.2). The L3/L1 skill premium ratio rose from its pre-treatment average of 2.548 to a post-treatment average of 3.436 (difference +0.888). However, the skill premium of the control group fell from a pre-treatment average of 2.555 to a post-treatment average of 2.306 (difference -0.249). The treatment effect is the difference-in-differences, i.e., $0.888 - (-0.249) = 1.137$.[16] In figure 3.16, which includes both the DID and the synthetic control group results, one of the important differences between these two approaches becomes clear: DID is a more crude measure of the liberalisation effect because it distinguishes only pre- and post-treatment averages and does not explain the development over time. Nonetheless, the DID results confirm those of the synthetic control group methodology.

Using the complete donor pool as the untreated group, the DID treatment effect becomes a bit smaller at 0.963 ($= 0.888 - (-0.075)$, see the last row of table 3.2).[17] In this case, the average skill premium of the control group falls by less, or, put differently, there is a larger pre-treatment difference between Mumbai and the control group, pointing to a

[16] See table D.1 in appendix D for the detailed regression results.
[17] See table D.2 in appendix D for the detailed regression results.

Table 3.2: DID results for skill premium ratio L3/L1

	1982-1991	1992-2009	Difference
(1) Mumbai	2.548	3.436	0.888
(2) Synthetic Control	2.555	2.306	-0.249
(3) Complete donor pool	2.762	2.687	-0.075
Difference (1)-(2)	-0.007	1.130	1.137
Difference (1)-(3)	-0.214	0.749	0.963

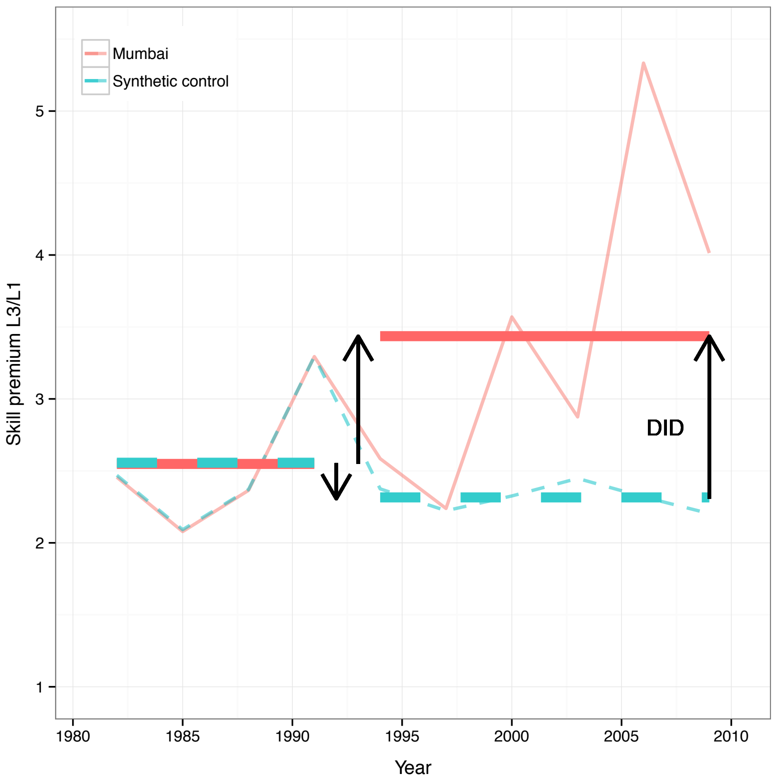


Figure 3.16: Skill premium path plots showing pre- and post-treatment means for Mumbai and its synthetic control for skill ratio L3/L1

potential problem when assuming equal time trends in these groups (see figure 3.17). For the synthetic control group discussed above, the pre-treatment difference is practically and statistically not distinguishable from zero.

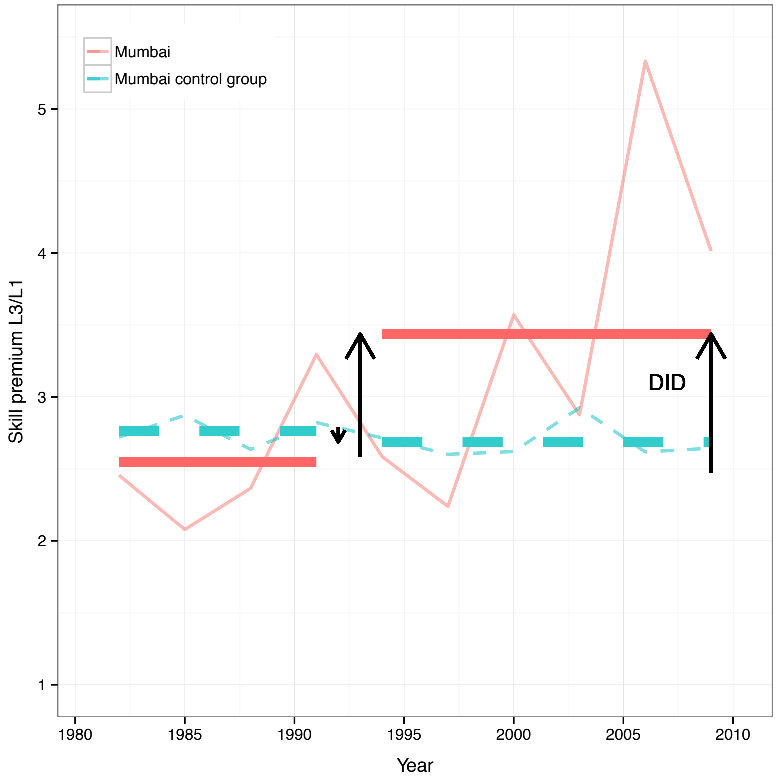


Figure 3.17: Skill premium path plots showing pre- and post-treatment means for Mumbai and the control group (complete donor pool) for skill ratio L3/L1

Difference-in-Differences results for L3/L2

For L3/L2, using the synthetic control group yields a DID estimate for the treatment effect of 0.382 (see table 3.3).[18] While the average skill premium of the control group rises slightly (+0.071), Mumbai's skill premium rises by more, leaving a still clear treatment effect in the L3/L2 skill premium, i.e., an increase in urban wage inequality. When the complete donor pool is used as the control group, the DID treatment effect is 0.477.[19] In this case, the average skill premium of the control group fell only very slightly (-0.024), and thus the full rise in Mumbai's skill premium is attributed to the treatment. Again, the

[18]See table D.3 in appendix D for the detailed regression results.
[19]See table D.4 in appendix D for the detailed regression results.

DID results broadly confirm our results from the synthetic control group methodology, but without providing detail regarding the development of the treatment effect over time.

Table 3.3: DID results for skill premium ratio L3/L2

	1982-1991	1992-2009	Difference
(1) Mumbai	1.399	1.852	0.453
(2) Synthetic Control	1.416	1.486	0.071
(3) Complete donor pool	1.689	1.665	-0.024
Difference (1)-(2)	-0.017	0.366	0.382
Difference (1)-(3)	-0.290	0.187	0.477

Difference-in-Differences results for L2/L1

For the L2/L1 skill premium ratio, we calculate the DID effect once for the post-treatment period 1994-2003 and once for 2006-2009 (see table 3.4). Using the synthetic control group and taking the data over the sample period 1982-2003, the DID estimate for the treatment effect is -0.550.[20] While the average skill premium of the control group rises (+0.244), Mumbai's post-treatment average L2/L1 skill premium falls from 1.851 to 1.545, resulting in a clearly negative treatment effect. In contrast, for the post-treatment period 2006-2009 Mumbai's average L2/L1 skill premium rose (+0.590), while the control group's fell (-0.238) relative to the pre-treatment levels, resulting in a treatment effect of +0.828.[21]

When the complete donor pool is used as the control group over the time period 1982-2003, the DID treatment effect is -0.322.[22] The rise in the average skill level of the complete donor pool is smaller than that of the synthetic control of the previous section, resulting in a slightly smaller treatment effect for Mumbai. For the post-treatment period 2006-2009 the DID treatment effect indicates a rise in the skill premium of 0.689.[23] As before, there is a difference between the DID effect in the complete donor pool and for the synthetic control group indicating a potential violation of the common trends assumption.

3.6 Discussion

Using data from the UBS Prices and Earnings surveys from 1982 to 2009, we find that in a first phase after the 1991 liberalisation in India medium-skilled wages fell relative

[20]See table D.5 in appendix D for the detailed regression results.
[21]See table D.6 in appendix D for the detailed regression results.
[22]See table D.7 in appendix D for the detailed regression results.
[23]See table D.8 in appendix D for the detailed regression results.

Table 3.4: DID results for skill premium ratio L2/L1

	1982-1991	1992-2003	Difference	2006-2009	Difference
(1) Mumbai	1.851	1.545	-0.306	2.441	0.590
(2) Synthetic Control	1.885	2.129	0.244	1.647	-0.238
(3) Complete donor pool	1.621	1.638	0.017	1.522	-0.099
Difference (1)-(2)	-0.034	-0.584	-0.550	0.794	0.828
Difference (1)-(3)	-0.230	-0.552	-0.322	0.459	0.689

to high- and low-skilled wages. Our synthetic control group shows that these losses were stronger than would have been expected if no reforms had been implemented. This implies a reduction in medium- to low-skilled wage inequality, but an increase in high- to medium-skilled inequality. From the late 1990s onwards, growth of high-skilled workers' wages outpaced both other groups. This suggests that after the first steps of liberalisation, new forces of globalization started to act on relative wages. We find some evidence that in a third phase, from the second half of the 2000s, medium-skilled wages accelerated and by 2009 had compensated for the previous losses relative to high-skilled wages. Versus the low-skilled, medium-skilled workers were even able to overcompensate earlier losses relative to what would have been expected without treatment. Of course, the further away from the initial steps of liberalisation in 1991 we move, the more sensitive the results get to secondary influences that, in fact, may have had little to do with India's economic liberalisation, and hence we place a cautionary mark on these very long-term results.

Regarding the medium-term impacts of India's economic liberalisation, our study points to a possible explanation of the diverging results in the previous literature. On the one hand, we confirm Kijima (2006) who found accelerating wage inequality between the high-skilled and low-skilled wages in the 1990s as measured by the 90th versus the 10th wage quantile. On the other hand, recalling the decline in medium-skilled relative to low-skilled wages during the initial post-liberalisation phase we also provide a potential explanation for Kumar and Mishra's (2008) result of falling wage inequality in some manufacturing sectors. While most studies distinguish only between skilled and unskilled workers, and group together a large number of different sectors, our results suggest that different levels of skill, such as in our case low-, medium- and high-skilled workers were likely impacted very differently. Therefore, a more disaggregated analysis seems likely to be more informative than the general definitions often used in the literature to define the skill premia, a promising direction for future research.

Regarding the methodological aspects of our study, we emphasise the importance of control group selection to suitably capture wage inequality trends under the counterfactual scenario of no liberalisation. Constructing a synthetic control group for a narrowly defined treatment region, namely the city of Mumbai, turns out to be a favourable approach to avoid a number of problems like urban/rural differences in sector compositions, diverging skill requirements across professions and countries, etc. While we used DID as a robustness check for the synthetic control group methodology, there are several differences between the two approaches that should be highlighted. First, the latter uses a data-driven process to create a control group which approximates the most important characteristics of the treated unit during the pre-treatment period. The composition of this synthetic control is transparent in that the contribution of each available control unit and the similarity (or lack thereof) of the synthetic control group to the treated unit before treatment is made explicit (Abadie et al., 2011, p. 2). Second, the synthetic control group allows for the effect of potentially unobserved characteristics that are not fixed over time, while DID only corrects for time-invariant characteristics. Third, standard DID uses two distinct points in time on which the comparison is based, usually averages over the periods before and after the treatment. If more pre- or post-treatment data is available such an approach neglects the additional time variation. The synthetic control group methodology is aimed at exactly that, i.e., observing the effects as they unfold over time.

As the selection of controls for DID estimation can be done in many ways, we confined ourselves to two examples: First, we used the synthetic control group as determined before and then calculated the treatment effect using standard DID. Second, we used all donor cities as the DID control. Our results suggest that by using the synthetic control group most of the pre-treatment differences to Mumbai disappear, whereas with the complete set of donor cities we observe important discrepancies in pre-treatment characteristics. These differences may be critical regarding the DID identifying assumption of equal time trends in the treatment and the control group (which possibly explains the different estimates of the treatment effect). While the DID comparison with the synthetic control group tends to be more credible than using all donor cities, with standard DID we still forego the detailed information on the differential time trends. For a critical evaluation of the impact of India's economic liberalisation on wage inequality, we therefore argue that the synthetic control group methodology is a suitable and promising approach. Even if one extends the basic DID framework to allow for flexible post-treatment trends, we still recommend using the synthetic control group methodology as a data pre-processing device to make the DID assumptions more plausible.

3.7 Conclusion

In this chapter we analyse the impact on the skill premium of far-reaching economic and trade liberalisation following India's dramatic balance-of-payments crisis in the early 1990s. As urban and rural areas are affected very differently by trade opening, we focus on urban wage inequality and the city of Mumbai, an important trading hub, using data from the UBS Prices and Earnings surveys from 1982 to 2009. We address the identification problem of the unobserved counterfactual outcome - i.e. the development of skill premia in Mumbai if no economic reforms had taken place - by employing the method of the synthetic control group, as proposed in Abadie and Gardeazabal (2003) and extended in Abadie, Diamond and Hainmueller (2010). We use a broad group of 35 other large cities, including many trading centres in emerging markets, to construct the synthetic control.

We see two main conclusions that can be drawn from our analysis. First, India's economic and trade liberalisation starting in the early 1990s had very differential impacts on skill premia, both over time and over skill levels. The most striking result probably is the large increase in wage inequality of high-skilled versus low-skilled professions. Second, the results from both the synthetic control group method and DID suggest that a significant part of this increase in wage inequality can be attributed to India's liberalisation. While DID provides rather uninformative results, the synthetic control group methodology suggests that this overall rise in inequality was not a one-way process because we find some periods in which measures of wage inequality were stable, or even falling. Overall, we emphasise the careful selection of a suitable control group in the analysis of liberalisation effects and encourage a more detailed analysis of wage inequality by multiple skill levels in the future.

Appendices

Appendix A

UBS Prices and Earnings Survey

This appendix introduces and describes the UBS Prices and Earnings dataset, in particular focussing on the data subset related to the labour market.[1] This dataset has been lauded from several corners,[2] but has not been exploited much in the academic literature. As this dataset has been reworked and reformatted and as detailed information is provided on it here, it is likely to become more accessible and valuable for future academic research.

A.1 Overview

In 1970 the Chief Economist of Union Bank of Switzerland, the forerunner to today's Swiss bank UBS, on a trip to New York realised the value of being able to compare the prices of goods and services internationally, especially within the context of local wage levels. Starting in 1970 the bank therefore commissioned its economics research department to do a survey of international prices and wages every three years, producing a dataset which displays remarkable consistency over the last 40 years. This consistency over time is backed by unusual global comparability, as in each country the survey was conducted contemporaneously with an identical questionnaire and comparable methods.

This time-consistency/global-comparability is the key distinguishing feature of the UBS Prices and Earnings dataset. In contrast, the vast majority of long-term international wage

[1] The information in this section is largely drawn from the original UBS Prices and Earnings survey questionnaires and publications.

[2] For instance, John O'Connor praises the UBS Prices and Earnings data and lists several of its characteristics that he believes provide a useful guide as to how the World Bank's International Comparison Program (ICP) could improve its approach. Referring to the ICP's calculation of PPP's he concludes that "(t)he weakest link at present is the income approach, which only UBS has tried to tackle" (O'Connor, 2008, p. 9).

comparisons are based on country data that stem from each country's national statistical offices, or other local sources. Data in these comparisons tend to vary from country to country exhibiting both known differences, for which adjustments can sometimes be made, and unknown differences that may never be identified. Many inconsistencies between the data of different countries are, for instance, due to differing compositions of markets, indexes, baskets, different definitions of prices, wages, and preferences, different time spans or dates of the surveys and different motives for data collection. These problems are side-stepped by the UBS approach of using identical questionnaires resulting in the measurement of a uniform basket of goods and services, and of the measurement of labour market data for comparable "representative" workers across the globe. The main drawback, clearly, is the lack of adaptation to local preferences and characteristics of local labour markets, which is discussed in more detail in section A.3.

A.2 Description: UBS Prices and Earnings Survey

From 1970 the UBS Prices and Earnings survey collected data every three years on approximately 122 standardised goods and services (approx. 95 goods, 27 services) and earnings data from professions in the manufacturing and services sector from 31 cities around the world in 1970, growing to 71 cities in 2009. The survey now yields over 30 000 data points which are merged into some 80 published aggregates per city.

The major variables surveyed in the "Earnings" section of the survey are gross annual wages, taxes and social security contributions, yielding net annual wages. Additionally, weekly working hours and days of paid annual holiday are collected. Professions surveyed were selected based on two criteria. First, they had to constitute a representative cross-section of the workforce in the manufacturing and service sectors. Second, the professions needed to be common in most metropolitan centres around the world and it had to be possible to define and consistently capture the data globally. The indicators include data for five professions in 1970, rising to twelve occupations in 1979, with fourteen professions surveyed in 2009. Except for the expansion in the number of cities and professions surveyed, the questionnaire on earnings has remained largely unchanged since 1970. However, data is not available for all cities in all years and especially the data on daily working hours and paid leave days is not complete for all professions and all years.

A.3 Methodology

As mentioned above, the main difference in methodology between the UBS Prices and Earnings data and other international datasets is the fact that the UBS data is based on a uniform survey across all cities, which has remained largely unchanged since 1970. The second significant difference is that UBS does not attempt to provide country estimates, but only provides data for selected cities, which in part justifies a smaller data sample. While it seems sensible to compare countries rather than cities, many databases used for country comparisons must often also make do with data primarily from urban areas – less affluent regions and rural areas are frequently poorly represented (cf. O'Connor, 2008, p. 3, and Van Ark and Monnikhof, 2000, p. 6ff). Thus, the UBS data is likely to prove quite similar to some country data, while being more explicit about its lack of country representativeness.

Conducting the Survey In each survey year UBS recruits local surveyors – mostly three to four independent surveyors could be found in each city through the bank's own network of employees, through correspondent banks, consumer organisations, chambers of commerce, universities and members of the student organisation AIESEC. The surveyors receive detailed instructions on how to proceed with the survey, including the time frame during which the survey is to be conducted,[3] for which types of businesses prices and wage data are to be collected, how value-added tax, rebates etc. should be treated, and which product quality range should be selected. With regard to the labour market information, surveyors are instructed to request data from representative companies of the specific sectors.

Once the survey data has been received, the data points are scanned for outliers. If outliers are found the relevant surveyor is contacted in order to clarify the relevant data point. If it is not possible to clarify the questions on the data point, it is omitted from further analysis. The remaining data for each survey item are averaged across surveyors. Aggregates are constructed for most categories within the "Prices" section of the surveys and for some sections, including the surveyed wages, the averaged values are rounded. In 1976 for the first time the data was evaluated "with the help of a computer", as is proudly stated in that year's publication, from 2000 onwards an Access database has been employed. The data for each survey year is published in the booklets "UBS Prices and Earnings Around the Globe", later simply "UBS Prices and Earnings" – these data form the basis of the dataset used in chapters 1 to 3. When a data range was provided in the

[3] See table A.1 on page 119 for the time frame of each survey.

Table A.1: Survey periods: UBS Prices and Earnings

Edition	Survey year	Survey months, if specified
1	1970	July
2	1973	July - August
3	1976	May - June
4	1979	June - July
5	1982	March - April
6	1985	March - April
7	1988	April - May
8	1991	April - May
9	1994	2nd quarter
10	1997	2nd quarter
11	2000	2nd quarter
12	2003	February - March
13	2006	February - April
14	2009	March

UBS Prices and Earnings publications, the mid-point of the range was selected as the data point for the dataset. If the mid-point seemed not to provide a plausible value (e.g. the data range was very wide), the data point was omitted.

The measures are provided in USD as converted from local currency using the average market exchange rate during the period of the survey. Expressing wage data in US dollars as converted by market exchange rates has the advantage of providing a clear-cut comparison of current prices expressed in current US dollars, allowing analyses across time and space. It does, however, not take into account differing price levels and therefore purchasing power in global comparisons. Therefore, for certain applications the UBS Prices and Earnings data must be adjusted to reflect purchasing power, by using appropriate conversion rates, for instance, those provided in the Penn World Tables.

A.4 Definitions of Indicators and Professions

In the following we provide the definitions of the labour market indicators in the UBS Prices and Earnings surveys and the precise descriptions of the professions surveyed in each year.

A.4.1 Indicators

Gross annual income Gross annual income (sum of hourly, weekly or monthly earnings) taking into account family status and tax allowances including all fringe benefits such as profit participation, bonuses, vacation money, additional monthly salaries as bonus payments, allowances for children etc., but excluding overtime compensation.

Taxes Amount of income taxes due (to federal and state or local authorities) on the listed annual gross income with allowance made for dependants and generally permitted deductions.

Social insurance contributions Mandatory contributions payable by employee (calculated on listed annual gross income) for government old age, survivors, disability and unemployment insurance and for government health insurance. Also included are employee contributions to company-sponsored health insurance, pension fund etc., if they are locally or nationally common practice.

Weekly working hours Normal number of working hours per week stipulated in employment contract, without overtime. In the case of the primary school teacher, the number of hours of instruction including the related preparatory hours are surveyed.

Note: Comparability of working hours of primary school teachers in the data is limited as some data include preparation time, while others only indicate the actual hours of school lessons. Information on paid vacation days and legal holidays is also patchy for this profession.

Paid vacations Number of paid working days per year.

Legal holidays Number of paid legal holidays per year.

Note: Paid vacation days and legal (public) holidays are aggregated and published as "vacation per year". These data have not been published in all editions of the UBS Prices and Earnings publication, but the data has been made available and is included in the dataset.

A.4.2 Professions

This section provides an overview of the professions surveyed. As mentioned above, professions surveyed were selected based on two criteria. First, they had to constitute a representative cross-section of the workforce in the manufacturing and service sectors. Second, the professions needed to be common in most metropolitan centres around the world and it had to be possible to define and consistently capture the data globally. While the descriptions mostly define gender, educational level, years of experience, approximate age and family status, these data are different to micro-economic household survey data in that they provide information from actual companies on representative agents, not actual agents.

Not all characteristics of the "representative agents" listed below are explicitly listed in the survey questionnaire for all professions. "NA" marks where they are not explicitly listed. Where the profession is considered to be primarily held by female workers, "implied female" is assumed, else "implied male" is assumed. The skills levels listed below are ascribed by the author (not the survey) based on the following breakdown:

Level 1: Obligatory / statutory schooling only; largely unskilled labour; very limited training.
Level 2: Obligatory / statutory schooling plus full apprenticeship or extensive practical training. Or completed high school and some practical training.
Level 3: Completed high school and university or college education.

Somewhat different training levels can exist for professions across countries so that this attribution is simplified, but it nonetheless provides a good first approximation in defining skill levels. The professions listed below are enumerated according to how long they have been included in the survey, with those included since 1970 appearing first.

1) **Primary school teacher:** Teaching in the state (also "public" / "government") school system (not private schools) for about 10 years; approx. 35 years old, married, two children.
Survey inclusion: Included in all surveys 1970 – 2009
Gender: NA / implied female
On the job experience in years: 10
Age: 35
Children: 2
Marital status: married
Skills level ascribed to profession: 3
Note: 1970: 5 years experience, aged approx. 30
Note: 1970-1992: no children

Note: Comparability of working hours of primary school teachers in the data is limited as some data include preparation time, while others only indicate the actual hours of school lessons. Information on paid vacation days and legal holidays is also patchy for this profession. For information on working hours for the other professions, see section A.5.2.

2) **Bus driver:** Employed by the municipal transportation system (also "public", or "municipal transport operator"), about 10 years driving experience; about 35 years old, married, two children.
Survey inclusion: Included in all surveys 1970 – 2009
Gender: NA / implied male
On the job experience in years: 10
Age: 35
Children: 2
Marital status: married
Skills level ascribed to profession: 1
Note: Fully consistent description over time.

3) **Automobile ("car") mechanic:** With completed apprenticeship and about 5 years of experience; 25 years old, single.
Survey inclusion: Included in all surveys 1970 – 2009
Gender: NA / implied male
On the job experience in years: 5
Age: 25
Children: 0
Marital status: single
Skills level ascribed to profession: 2
Note: Fully consistent description over time.

4) **Construction worker / building labourer:** Unskilled or semi-skilled labourer; about 25 years old, single.
Survey inclusion: Included in all surveys 1970 – 2009
Gender: NA / implied male
On the job experience in years: NA
Age: 25
Children: 0
Marital status: single

Skills level ascribed to profession: 1
Note: Fully consistent description over time.

5) **Bank teller or bank credit clerk:** Completed bank training and has about 10 years of experience (in a bank); approx. 35 years old, married with 2 children.
Survey inclusion: Included in surveys 1970 – 2009.
Gender: NA / implied male
On the job experience in years: 10
Age: 35
Children: 2
Marital status: married
Skills level ascribed to profession: 2
Note: 1970-1991: "Bank teller" (but detailed description as above)
Note: 1994-2009: "Bank credit clerk / officer" (but detailed description as above)

6) **Secretary:** Secretary ("personal assistant") to a department manager in an industrial company ("firm" or "organisation"), about 5 years experience (shorthand, typing, PC skills ("computer knowledge"), one foreign language); about 25 years old, single.
Survey inclusion: Included in surveys 1970 – 2009
Gender: female
On the job experience in years: 5
Age: 25
Children: 0
Marital status: single
Skills level ascribed to profession: 2
Note: 1970: 3 years experience, around 22 years old
Note: 1970-1991: no "PC skills" or "computer knowledge" required
Note: 1994-2000: "industrial or commercial company"
Note: 2003-2009: "industrial or service company"; "shorthand" not required anymore

7) **Department manager in industry:** Operational head ("operations manager", "technical manager", or just "head") of a production department (more than 100 employees) in a sizable company of the metal working industry; completed vocational training with many years of experience in the field; about 40 years old, married, two children.
Survey inclusion: Included in surveys 1973 – 2009.
Gender: NA / implied male

On the job experience in years: 15+
Age: 40
Children: 2
Marital status: married
Skills level ascribed to profession: 3
Note: 1973: "Personnel Manager in an industrial organisation with about 1000 employees"; one child
Note: 1976-1991: no children

8) **Skilled industrial worker:** Skilled mechanic ("worker") with vocational training and about 10 years experience with a large company in the metal working industry; about 35 years old, married, two children.
Survey inclusion: Included in surveys 1976 – 2009.
Gender: NA / implied male
On the job experience in years: 10
Age: 35
Children: 2
Marital status: married
Skills level ascribed to profession: 2
Note: 1976-1988: "Toolmaker / Lathe operator" (but detailed description as above)
Note: 1991: "Machinist" (but detailed description as above)

9) **Female factory worker:** Unskilled or semi-skilled (machine) operator in a medium-sized company ("plant", "mill"), mainly in the textile industry; about 25 years old, single.
Survey inclusion: Included in surveys 1976 – 2009
Gender: female
On the job experience in years: NA
Age: 25
Children: 0
Marital status: single
Skills level ascribed to profession: 1
Note: 1976-1991: "Textile worker (female)" (but detailed description as above)
Note: 1994-2000: "Industrial worker (female)" (but detailed description as above)

10) **Female sales assistant:** Female sales assistant ("Saleswoman") employed in a women's clothing section ("ladies wear") of a large department store; sales training plus

some years of sales experience; about 20 to 25 years old, single.
Survey inclusion: Included in surveys 1979 – 2009
Gender: female
On the job experience in years: 5
Age: 20-25
Children: 0
Marital status: single
Skills level ascribed to profession: 1
Note: 1979-1991: aged 20 to 24
Note: 1979-1991: some training but not specifically sales training

11) **Electrical engineer:** Employed by an industrial firm in the electrical engineering sector; university or technical college graduate with at least 5 years' of work experience, about 35 years old, married, two children.
Survey inclusion: Included in surveys 1979 – 2009.
Gender: NA / implied male
On the job experience in years: 5-10
Age: 35
Children: 2
Marital status: married
Skills level ascribed to profession: 3
Note: 1979-1991: no children
Note: 1997-2009: "Engineer", "electrical" not explicitly mentioned
Note: 1979-2003; "in the machinery or electrical equipment industry, electric power company or similar"

12) **Cook:** Works in the kitchen of a good restaurant or hotel with a fairly large staff. His position is that of the deputy of the Chief Cook or Chef de Partie, supervising 2-3 cooks; completed vocational training as cook and has about 10 years of experience; approx. 30 years old, single. / Salary data include value of free meals and lodging, if such are provided.
Survey inclusion: Included in surveys 1979 – 2009
Gender: NA / implied male
On the job experience in years: 10
Age: 30
Children: 0

Marital status: single
Skills level ascribed to profession: 2

13) **Product Manager:** Employed in the pharmaceuticals, chemicals or food industry, middle-management position, university or technical college graduate with at least 5 years' experience in the field; about 35 years old, married, no children.
Survey inclusion: Included in surveys 2003 – 2009
Gender: NA / implied female
On the job experience in years: 5+
Age: 35
Children: 0
Marital status: married
Skills level ascribed to profession: 3
Note: Fully consistent description over time.

14) **Call centre agents:** Trained agent at an inbound call / service centre, e.g. in the telecommunications or technology sector, about 25 years old, single.
Survey inclusion: Included in surveys 2006 – 2009
Gender: NA / implied male
On the job experience in years: NA
Age: 25
Children: 0
Marital status: single
Skills level ascribed to profession: 2
Note: Fully consistent description over time.

A.5 Important Characteristics, Strengths and Limitations

"*A more mundane but no less important lesson learned is that data issues are important. One cannot reliably know what data tell us if one does not first understand data limitations and strengths. Such an understanding requires thorough knowledge of how data surveys are constructed and attention to a host of measurement and estimation issues.*"
Barry T. Hirsch (2008, p. 16)

In the following sections we review the strengths and limitations of the UBS data, often juxtaposing with characteristics of alternative sources of data for international comparisons.

A.5.1 Strengths

Time consistency and international comparability As discussed above, these are the key distinguishing features of the UBS Prices and Earnings data – and also their most important strength. Linked to this is the advantage that no third party sources are involved.

No third party sources involved The UBS Prices and Earnings dataset in itself does not rely on third party statistical sources, or on data that has been collected through differing systems and using diverging methods, such as national or labour account statistics, or household or firm-level surveys. It sidesteps the problems of differing compositions of indexes, baskets, different definitions of prices, wages, and preferences, differing structures of markets, different time spans or dates of the surveys and different motives for data collection.

These present major hurdles to many alternative data sources. While national account statistics are largely harmonised internationally, they still differ sharply in nature and quality of the statistics. Labour accounts, which are usually derived from population or labour force surveys, are mostly even less harmonised internationally than national accounts systems (see Van Ark and Monnikhof, 2000, p. 4). Salverda, Nolan, and Smeeding (2009, p. 76) mention a number of factors that "impinge on data comparability" in micro-level data, for instance, differences in the source of data (e.g. household surveys, administrative archives, income tax records), the treatment of people entering and leaving the population, bottom- and top-coding of data in the course of collection of the data (see, for instance, Ryscavage, 1995, regarding the US Current Population Survey) or by researchers aiming to reduce noise (Cowell and Victoria-Feser, 1996; Burkhauser et al., 2007), and problematic methods of ranking household survey observations. Goldberg and Pavcnik (2004, p. 5) remind of the scrutiny households surveys have come under due to the "suspected increase in the non-response rates of richer households", which in particular make them less suitable for assessing inequality. In terms of international comparisons they emphasise that "the design of the surveys from developing countries often changes from year to year, making comparisons across years difficult", citing Colombia as an example (Goldberg and Pavcnik, 2004, p. 6).[4] A further alternative to the UBS data could be seen in firm level data. In these the information on worker and job-type characteristics is, however, mostly much more limited than both in household surveys and the UBS data, resulting in researchers frequently only being able to distinguish

[4]See the papers on poverty measurement by Ravallion (2003) and Deaton (2003) for a detailed discussion on the inconsistencies in the design of household surveys.

between production and non-production workers. Goldberg and Pavcnik (2004, p. 20) see this lack of accuracy in the measurement of skills as a major drawback of firm-level data.

Many measurement problems are also known to exist in data that has been assimilated from different sources, for instance, difficulties in taking into account the inconsistently measured services sector, or the diverging weighting systems, which are frequently adjusted at different intervals (see, for instance, Van Ark and Monnikhof, 2000, p. 2). These differences and inconsistencies are particularly important, when levels are compared, as described below.

Levels, as well as growth rates When growth rates are compared over time – if distortions remain constant over the period – the impact of errors is more limited. Maddison (1996), for example, suggests that over the past 40 to 50 years distortions in growth measures of OECD countries due to non-comparability of the data account for "less than 0.2% a year (and) should probably not be regarded as significant" (Maddison, 1996, p. 30). While levels are more sensitive to errors they clearly are more informative and are useful in a larger number of applications. The fact that the UBS data is available in level terms, and not only in growth rates, distinguishes it from other databases.

USD or local currency The original UBS Prices and Earnings data is provided in current USD as converted from local currencies using the average market exchange rates during the periods of each survey. However, local currency values can be calculated based on the original conversion rates.

Wages include private sector employment Mainly due to confidentially concerns and difficulties in surveying, many data sources provide only data on public sector wages (if wage data is supplied at all), omitting private sector professions. As public sector wages are often not representative of the labour market, e.g. due to factors such as generous pension and holiday allowances, and higher job security, the provision of data on private sector employment in the UBS data is another positive.

A.5.2 Limitations

Uniform basket Many researchers who have specialised in international data comparisons will see the UBS Prices and Earnings data's largest weakness in its use of a uniform basket of goods, services and professions across countries. Clearly, the lack of adaptation to local preferences, idiosyncrasies and market characteristics can be seen as a strong drawback. If the desire is to compare baskets that in each case are adapted to the local environment, then comparing data from the national statistical offices will usually be more appropriate. In this sense, the UBS data fills a different gap: For the labour market survey professions were selected that exist in most large cities and are representative of both the service and manufacturing sectors – for these professions the UBS data is highly comparable. However, clearly, the data does not provide for differences in the structure of labour markets, as household survey data would. This means that this wage data cannot be assumed to represent all professions within a city.

Gerschenkron effects Related to the selection of a uniform basket is the possible occurrence of Gerschenkron effects: The negative correlation between prices and volumes in consumer demand patterns means that the overrepresentation of less common goods can result in indices being biased (see, for instance, Jonas and Sardy, 1970). However, this difficulty is not specific to the UBS Prices and Earnings data; rather it is a pertinacious problem of index numbers that commonly arises especially when comparing economies at different levels of development, or in rapid development.

Cities, not countries The UBS surveys are done in specific cities only, and do not necessarily reflect each country's situation as a whole. While it would be more useful to compare countries rather than cities, many databases used for country comparisons must often also make do with data primarily from urban areas – less affluent regions and rural areas are frequently poorly represented (see, for instance, O'Connor, 2008, p. 3, and Van Ark and Monnikhof, 2000, p. 6ff).

Frequency Conducted only every three years, the UBS Prices and Earnings survey provides earnings data that will not easily show up short-run changes in relative wages, e.g. due to economic shocks, as these might not be captured. Rather, the data are likely to show up long run, general equilibrium effects that spread over several years (e.g. wage effects stemming from the dynamic indirect link from trade, to growth, to income).

No reflection of total labour costs for employers Labour compensation data in the UBS Prices and Earnings dataset show representative gross and net earnings of employees for different professions in current prices. The data, however, do not reflect total labour costs of employers, as taxes and social security contributions that are paid directly by the employer to the state, as well as other labour costs (such as insurances) are not captured.

Manufacturing and services, not agriculture As the surveys are undertaken in major metropolitan centres around the world, professions were selected to be representative of both the manufacturing and services sectors (with about half of professions in either sector), and based on the existence and comparability of these professions across major cities. No data from the agricultural sector is included. Thus, in particular for countries with large agricultural sectors, the wage data of the UBS Prices and Earnings surveys should not be assumed to reflect the country as a whole.

The two above data characteristics are largely comparable with the characteristics of labour market data based on wage indices (in contrast to full compensation measures), as wage or earnings indices typically exclude employers' contributions to tax and social security and often refer to certain types of workers, or specific economic activities, usually excluding the agricultural sector.[5]

Sampling and data gaps A weakness of the UBS data is the fact that for each data point published usually only three to four sampling points will have been gathered, i.e. in each city three to four independent surveyors will have provided one sampling point each. This is only slightly improved by the screening for and exclusion of outliers. Also, the data shows significant data gaps: The first is the surprising inconsistency in the selection of cities for each survey – cities will sometimes have appeared in the survey for many years, then suddenly disappear (e.g. Panama City 1976-2000, Houston 1988-2000, Düsseldorf 1970-1994), while others will only be included for one specific year, and never again (e.g. Vaduz 2006, Lugano 2003, Basel 2003). While explanations are sometimes given, e.g. Berlin replaced Düsseldorf after the fall of the Berlin wall, it becomes clear from these selections that the surveys were each time focusing on providing a once-off comparison of data around the globe, and not intentionally working on building time series data. Of the 35 cities that appeared in every, or nearly every survey, 17 are in Europe, six in Latin America, five in North America, four in Asia, one in Africa, one in the Middle East and one in Australia. Table A.3 on pages 132 to 134 shows which cities were included in which survey years.

The second significant data gap is the data on working hours and number of holidays for primary school teachers. While weekly working hours for this profession are not provided at all in 1970, the subsequent publications mostly state that where weekly working hours are given for teachers, comparability is limited as preparation work was sometimes excluded, sometimes included.

[5] Cf. Van Ark and Monnikhof (2000, p. 6ff).

Comparability of working hours Looking at working hours more broadly, the UBS survey measures paid working hours (i.e. those contractually agreed), thus unpaid and paid overtime is generally excluded. This problem is encountered in all attempts at international comparisons of working hours and Van Ark and Monnikhof (2000, p. 4), for example, state that "(c)omprehensive estimates of annual working hours are difficult to obtain and their international comparability is limited", in particular citing the difficulty in measuring hours actually worked versus paid hours. While not reflecting "the whole truth", paid working hours measured by UBS vary sharply between countries and do provide very insightful information as to the working conditions of actual labour markets.

A.6 Conclusion

The UBS Prices and Earnings dataset is unique in its high comparability across a broad range of countries and its consistency over time. Nonetheless, this data is not suitable for all applications and strengths and limitations must be considered before selecting this dataset as a source.

Table A.3: City inclusion in UBS Prices and Earnings surveys, page 1 of 3

No.	City	Country	1970	1973	1976	1979	1982	1985	1988	1991	1994	1997	2000	2003	2006	2009	Total
1	Abu Dhabi	UAE	0	0	0	1	1	1	1	0	1	1	1	0	0	0	7
2	Amsterdam	Netherlands	1	1	1	1	1	1	1	1	1	1	1	1	1	1	14
3	Athens	Greece	1	1	1	1	1	1	1	1	1	1	1	1	1	1	14
4	Auckland	New Zealand	0	0	0	0	0	0	0	0	0	0	1	1	1	1	4
5	Bangkok	Thailand	0	0	0	1	1	1	1	0	1	1	1	1	1	1	10
6	Barcelona	Spain	0	0	0	0	0	0	0	0	0	0	1	1	1	1	4
7	Basel	Switzerland	0	0	0	0	0	0	0	0	0	0	0	1	0	0	1
8	Beijing	China	0	0	0	0	0	0	0	0	0	0	0	0	0	1	1
9	Beirut	Lebanon	1	1	0	0	0	0	0	0	0	0	0	0	0	0	2
10	Berlin	Germany	0	0	0	0	0	0	0	0	0	1	1	1	1	1	5
11	Bogota	Colombia	1	1	1	1	1	1	1	1	1	1	1	1	1	1	14
12	Bratislava	Slovakia	0	0	0	0	0	0	0	0	0	0	0	0	0	1	1
13	Brussels	Belgium	1	1	1	1	1	1	1	1	1	1	1	1	1	1	14
14	Bucharest	Romania	0	0	0	0	0	0	0	0	0	0	0	0	0	1	1
15	Budapest	Hungary	0	0	0	0	0	0	0	0	1	1	1	1	1	1	6
16	Buenos Aires	Argentina	1	1	1	1	1	1	1	1	1	1	1	1	1	1	14
17	Cairo	Egypt	0	0	0	0	1	1	1	1	1	1	1	0	0	1	8
18	Caracas	Venezuela	0	1	1	1	1	1	1	1	1	1	1	1	1	1	13
19	Chicago	USA	1	1	1	1	1	1	1	1	1	1	1	1	1	1	14
20	Copenhagen	Denmark	1	1	1	1	1	1	1	1	1	1	1	1	1	1	14
21	New Delhi	India	0	0	0	0	0	0	0	0	0	0	0	0	0	1	1
22	Doha	Quatar	0	0	0	0	0	0	0	0	0	0	0	0	0	1	1
23	Dubai	UAE	0	0	0	0	0	0	0	0	0	0	0	0	0	1	1
24	Dublin	Ireland	0	0	1	1	1	1	1	1	1	1	1	1	1	1	12
25	Duesseldorf	Germany	1	1	1	1	1	1	1	1	1	0	0	0	0	0	9
26	Frankfurt	Germany	0	0	0	0	0	0	1	1	1	1	1	1	1	1	8
27	Geneva	Switzerland	1	1	1	1	1	1	1	1	1	1	1	1	1	1	14
28	Helsinki	Finland	1	1	1	1	1	1	1	1	1	1	1	1	1	1	14
29	Hong Kong	Hong Kong	1	1	1	1	1	1	1	1	1	1	1	1	1	1	14

City inclusion in UBS Prices and Earnings surveys, page 2 of 3

No.	City	Country	1970	1973	1976	1979	1982	1985	1988	1991	1994	1997	2000	2003	2006	2009	Total
30	Houston	USA	0	0	0	0	0	1	1	1	1	1	1	0	0	0	6
31	Istanbul	Turkey	0	1	1	1	1	1	1	0	0	1	1	1	1	1	11
32	Jakarta	Indonesia	0	0	0	1	1	1	1	1	1	1	1	1	1	1	11
33	Jedda (Jiddah)	Saudi-Arabia	0	0	0	1	1	1	1	0	0	0	0	0	0	0	4
34	Johannesburg	South Africa	1	1	1	1	1	1	1	1	1	1	1	1	1	1	14
35	Karachi	Pakistan	0	0	0	0	0	0	0	0	0	0	0	1	0	0	1
36	Kiev	Belarus	0	0	0	0	0	0	0	0	0	0	0	0	0	1	1
37	Kuala Lumpur	Malaysia	0	0	0	0	0	1	1	1	1	1	1	1	1	1	9
38	Lagos	Nigeria	0	0	0	0	0	1	1	1	1	0	0	1	0	0	5
39	Lima	Peru	0	0	0	0	0	0	0	0	0	0	0	0	0	1	1
40	Lisbon	Portugal	1	1	1	1	1	1	1	1	1	1	1	1	1	1	14
41	Ljubljana	Slovenia	0	0	0	0	0	0	0	0	0	0	0	0	0	1	1
42	London	UK	1	1	1	1	1	1	1	1	1	1	1	1	1	1	14
43	Los Angeles	US	0	0	1	1	1	1	1	1	1	1	1	1	1	1	12
44	Lugano	Switzerland	0	0	0	0	0	0	0	0	0	0	0	1	0	0	1
45	Luxembourg	Luxembourg	1	1	1	1	1	1	1	1	1	1	1	1	1	1	14
46	Lyon	France	0	0	0	0	0	0	0	0	0	0	0	0	0	1	1
47	Madrid	Spain	1	1	1	1	1	1	1	1	1	1	1	1	1	1	14
48	Manama	Bahrain	0	0	1	1	1	1	1	0	1	1	1	1	1	1	11
49	Manila	Philippines	0	0	1	1	1	1	1	1	1	1	1	1	1	1	12
50	Mexico City	Mexico	1	1	1	1	1	1	1	1	1	1	1	1	1	1	14
51	Miami	USA	0	0	0	0	0	0	0	0	0	0	0	0	0	1	1
52	Milan	Italy	1	1	1	1	1	1	1	1	1	1	1	1	1	1	14
53	Montreal	Canada	1	1	1	1	1	1	1	1	1	1	1	1	1	1	14
54	Moscow	Russia	0	0	0	0	0	0	0	0	0	1	1	1	1	1	5
55	Mumbai (Bombay)	India	0	1	0	0	1	1	1	1	1	1	1	1	1	1	11
56	Munich	Germany	0	0	0	0	0	0	0	0	0	0	0	0	0	1	1
57	Nairobi	Kenya	0	0	0	0	0	0	1	1	1	1	1	1	1	1	8
58	New York	USA	1	1	1	1	1	1	1	1	1	1	1	1	1	1	14

City inclusion in UBS Prices and Earnings surveys, page 3 of 3

No.	City	Country	1970	1973	1976	1979	1982	1985	1988	1991	1994	1997	2000	2003	2006	2009	Total
59	Nicosia	Cyprus	0	0	0	0	0	0	1	1	1	1	1	0	1	1	7
60	Oslo	Norway	1	1	1	1	1	1	1	1	1	1	1	1	1	1	14
61	Panama City	Panama	0	0	1	1	1	1	1	1	1	1	1	0	0	0	9
62	Paris	France	1	1	1	1	1	1	1	1	1	1	1	1	1	1	14
63	Prague	Czech Republic	0	0	0	0	0	0	0	0	1	1	0	1	1	1	5
64	Riga	Latvia	0	0	0	0	0	0	0	0	0	0	0	0	0	1	1
65	Rio de Janeiro	Brazil	1	1	1	1	1	1	1	1	1	1	1	1	1	1	14
66	Rome	Italy	1	1	0	0	0	0	0	0	0	0	0	1	1	1	5
67	San Francisco	USA	0	1	1	1	1	0	0	0	0	0	0	0	1	0	5
68	Santiago de Chile	Chile	0	0	0	0	0	0	0	0	0	0	1	1	1	1	4
69	Sao Paulo	Brazil	1	1	1	1	1	1	1	1	1	1	1	1	1	1	14
70	Seoul	South Korea	0	0	0	0	1	1	1	1	1	1	1	1	1	1	10
71	Shanghai	China	0	0	0	0	0	0	0	0	0	1	1	1	1	1	5
72	Singapore	Singapore	0	1	1	1	1	1	1	1	1	1	1	1	1	1	13
73	Sofia	Bulgaria	0	0	0	0	0	0	0	0	0	0	0	0	0	1	1
74	Stockholm	Sweden	1	1	1	1	1	1	1	1	1	1	1	1	1	1	14
75	Sydney	Australia	1	1	1	1	1	1	1	1	1	1	1	1	1	1	14
76	Taipei	Taiwan	0	0	0	0	0	0	0	1	1	1	1	1	1	1	7
77	Tallinn	Estonia	0	0	0	0	0	0	0	0	0	0	0	0	0	1	1
78	Tehran	Iran	0	0	1	1	0	0	0	0	0	0	0	0	0	0	2
79	Tel Aviv	Israel	0	1	1	1	1	1	1	1	1	1	1	1	1	1	13
80	Tokyo	Japan	1	1	1	1	1	1	1	1	1	1	1	1	1	1	14
81	Toronto	Canada	0	0	1	1	1	1	1	1	1	1	1	1	1	1	12
82	Vaduz	Liechtenstein	0	0	0	0	0	0	0	0	0	0	0	0	1	0	1
83	Vienna	Austria	1	1	1	1	1	1	1	1	1	1	1	1	1	1	14
84	Vilnius	Lithuania	0	0	0	0	0	0	0	0	0	0	0	0	0	1	1
85	Warsaw	Poland	0	0	0	0	0	0	0	0	0	1	1	1	1	1	5
86	Zurich	Switzerland	1	1	1	1	1	1	1	1	1	1	1	1	1	1	14
	Total		31	37	41	45	47	49	52	48	53	56	58	59	58	73	Total

Appendix B

Appendix for Chapter 1

Table B.1: Profession-level wage growth regressed on initial real GDP per capita with city fixed effects for 1970-2009, and the sub-periods 1970-1988 and 1988-2009 (Amsterdam=baseline)

	(1) OLS + Fixed effects (City) 1970-2009 Total period (balanced)	(2) OLS + Fixed effects (City) 1970-1988 Estimated separately (unbalanced)	(3) OLS + Fixed effects (City) 1988-2009 Estimated separately (unbalanced)
Intercept 1/ 2	-0.065 (0.108)	-0.212 (0.205)	-0.016 (0.282)
Ln GDP/capita 1970 / 1988	0.010 (0.011)	0.028 (0.020)	0.003 (0.027)
factor(City)Athens	0.017 (0.008)*	0.018 (0.017)	0.012 (0.019)
factor(City)Bangkok			0.029 (0.058)
factor(City)Bogota	0.025 (0.021)	0.052 (0.039)	0.007 (0.047)
factor(City)Brussels	0.011 (0.006).	0.023 (0.011)*	-0.001 (0.008)
factor(City)Buenos Aires	0.027 (0.013)*	0.002 (0.025)	0.050 (0.037)
factor(City)Cairo			0.090 (0.069)
factor(City)Caracas			-0.010 (0.033)
factor(City)Chicago	0.003 (0.004)	0.005 (0.008)	0.006 (0.006)
factor(City)Copenhagen	0.007 (0.005)	0.009 (0.010)	0.004 (0.007)
factor(City)Dublin			0.019 (0.017)
factor(City)Duesseldorf		0.011 (0.010)	
factor(City)Frankfurt			0.003 (0.007)
factor(City)Geneva	-0.002 (0.004)	-0.002 (0.008)	-0.003 (0.007)
factor(City)Helsinki	0.015 (0.007)*	0.019 (0.013)	0.013 (0.008).
factor(City)Hong Kong	0.039 (0.014)**	0.071 (0.026)**	0.014 (0.010)
factor(City)Istanbul			-0.001 (0.043)
factor(City)Jakarta			0.021 (0.075)
factor(City)Johannesburg	0.000 (0.017)	0.015 (0.032)	0.003 (0.046)
factor(City)Kuala Lumpur			0.036 (0.049)
factor(City)Lisbon	0.015 (0.013)	0.012 (0.024)	0.033 (0.021)
factor(City)London	0.020 (0.007)**	0.036 (0.013)**	0.009 (0.009)
factor(City)Los Angeles			0.009 (0.006)
factor(City)Luxembourg	0.012 (0.004)**	0.018 (0.007)*	0.003 (0.010)
factor(City)Madrid	0.008 (0.008)	0.004 (0.016)	0.020 (0.013)
factor(City)Manama			0.001 (0.015)
factor(City)Manila			0.013 (0.071)
factor(City)Mexico City	-0.021 (0.014).	-0.047 (0.027)	0.016 (0.033)
factor(City)Milan	0.006 (0.007)	0.017 (0.013).	0.001 (0.008)
factor(City)Montreal	0.004 (0.005)	0.008 (0.009)	-0.002 (0.006)
factor(City)Mumbai			0.037 (0.084)
factor(City)Nairobi			0.019 (0.089)
factor(City)New York	0.006 (0.004)	-0.008 (0.008)	0.020 (0.006)**
factor(City)Nicosia			0.025 (0.023)
factor(City)Oslo	0.008 (0.005)	0.003 (0.009)	0.009 (0.006)
factor(City)Paris	0.014 (0.006)*	0.017 (0.011)	0.013 (0.007).
factor(City)Rio de Janeiro	0.002 (0.020)	-0.004 (0.038)	0.017 (0.041)
factor(City)Rome	-0.000 (0.007)		
factor(City)Sao Paulo	0.010 (0.020)	0.023 (0.038)	0.020 (0.041)
factor(City)Seoul			0.020 (0.031)
factor(City)Singapore			0.021 (0.011).
factor(City)Stockholm	-0.002 (0.004)	-0.018 (0.008)*	0.010 (0.007)
factor(City)Sydney	0.009 (0.004)*	0.005 (0.008)	0.015 (0.006)*
factor(City)Tel Aviv			0.019 (0.015)
factor(City)Tokyo	0.017 (0.007)*	0.032 (0.013)*	0.008 (0.007)
factor(City)Toronto			0.001 (0.006)
factor(City)Vienna	0.010 (0.006).	0.021 (0.011).	0.003 (0.007)
factor(City)Zurich	-0.001 (0.004)	-0.001 (0.008)	-0.001 (0.007)
Multiple R^2	0.76	0.79	0.48
Adjusted R^2	0.70	0.74	0.43
Num. obs.	144	141	539

Notes: The term "balanced" indicates that only cities were included, for which data was available for both periods 1970-1988 and 1988-2009, or for the total period 1970-2009. In the "unbalanced" panels additional cities, for which data was only available in one of the periods were added to benefit from the larger sample size. The standard errors are shown in brackets.

$^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$, $p < 0.1$

Table B.2: Wage regression estimates jointly for the periods 1970-1988 and 1988-2009 on profession-level data, including the fertility rate and education expenditure as steady state control variables, and country fixed effects (Argentina=baseline)

	Solow-Swan conditional convergence with two steady state control variables and country fixed effects
(Intercept)	$0.253\ (0.013)^{***}$
log(InitialWagePPP)	$-0.023\ (0.001)^{***}$
EduExpenditurePercGNI	$-0.006\ (0.001)^{***}$
FertilityRate	$0.001\ (0.002)$
factor(Country)Australia	$0.032\ (0.007)^{***}$
factor(Country)Austria	$0.031\ (0.007)^{***}$
factor(Country)Bahrain	$0.008\ (0.009)$
factor(Country)Belgium	$0.029\ (0.007)^{***}$
factor(Country)Brazil	$-0.001\ (0.007)$
factor(Country)Canada	$0.040\ (0.009)^{***}$
factor(Country)Colombia	$-0.001\ (0.010)$
factor(Country)Cyprus	$0.021\ (0.007)^{**}$
factor(Country)Denmark	$0.043\ (0.009)^{***}$
factor(Country)Egypt	$0.035\ (0.008)^{***}$
factor(Country)Finland	$0.034\ (0.008)^{***}$
factor(Country)France	$0.029\ (0.007)^{***}$
factor(Country)Greece	$0.008\ (0.010)$
factor(Country)Hong Kong	$0.017\ (0.006)^{**}$
factor(Country)India	$0.001\ (0.008)$
factor(Country)Indonesia	$-0.035\ (0.007)^{***}$
factor(Country)Ireland	$0.037\ (0.009)^{***}$
factor(Country)Israel	$0.031\ (0.009)^{***}$
factor(Country)Italy	$0.024\ (0.007)^{**}$
factor(Country)Japan	$0.031\ (0.007)^{***}$
factor(Country)Kenya	$0.003\ (0.012)$
factor(Country)Korea, Republic of	$0.015\ (0.007)^{*}$
factor(Country)Luxembourg	$0.027\ (0.007)^{***}$
factor(Country)Malaysia	$0.019\ (0.008)^{*}$
factor(Country)Mexico	$-0.027\ (0.007)^{***}$
factor(Country)Netherlands	$0.037\ (0.009)^{***}$
factor(Country)Norway	$0.036\ (0.008)^{***}$
factor(Country)Philippines	$-0.029\ (0.007)^{***}$
factor(Country)Portugal	$0.018\ (0.007)^{**}$
factor(Country)Singapore	$0.008\ (0.007)$
factor(Country)South Africa	$0.010\ (0.009)$
factor(Country)Spain	$0.015\ (0.006)^{*}$
factor(Country)Sweden	$0.035\ (0.009)^{***}$
factor(Country)Switzerland	$0.029\ (0.006)^{***}$
factor(Country)Thailand	$0.003\ (0.007)$
factor(Country)Turkey	$-0.017\ (0.007)^{*}$
factor(Country)United Kingdom	$0.035\ (0.007)^{***}$
factor(Country)United States of America	$0.042\ (0.007)^{***}$
factor(Country)Venezuela	$-0.023\ (0.007)^{**}$
R^2	0.618
Adj. R^2	0.592
Num. obs.	646

$^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$, $^{\cdot}p < 0.1$

Table B.3: Wage regression estimates jointly for the periods 1970-1988 and 1988-2009 on profession-level data, including the fertility rate, education expenditure, life expectancy at birth and investment share of GDP as steady state control variables, and country fixed effects (Argentina=baseline)

	Solow-Swan conditional convergence with four steady state control variables and country fixed effects
(Intercept)	0.708 (0.076)***
log(InitialWagePPP)	−0.019 (0.001)***
EduExpenditurePercGNI	−0.007 (0.001)***
FertilityRate	−0.016 (0.004)***
LifeExpAtBirth	−0.006 (0.001)***
InsShareGDP	−0.002 (0.001)***
factor(Country)Australia	0.054 (0.009)***
factor(Country)Austria	0.034 (0.008)***
factor(Country)Bahrain	0.053 (0.012)***
factor(Country)Belgium	0.037 (0.008)***
factor(Country)Brazil	−0.025 (0.008)**
factor(Country)Canada	0.050 (0.009)***
factor(Country)Colombia	−0.012 (0.010)
factor(Country)Cyprus	0.059 (0.010)***
factor(Country)Denmark	0.044 (0.009)***
factor(Country)Egypt	0.002 (0.010)
factor(Country)Finland	0.048 (0.011)***
factor(Country)France	0.039 (0.007)***
factor(Country)Greece	0.024 (0.011)*
factor(Country)Hong Kong	0.046 (0.009)***
factor(Country)India	−0.049 (0.011)***
factor(Country)Indonesia	−0.068 (0.009)***
factor(Country)Ireland	0.048 (0.009)***
factor(Country)Israel	0.049 (0.009)***
factor(Country)Italy	0.035 (0.008)***
factor(Country)Japan	0.068 (0.011)***
factor(Country)Kenya	−0.012 (0.012)
factor(Country)Korea, Republic of	0.018 (0.012)
factor(Country)Luxembourg	0.026 (0.008)***
factor(Country)Malaysia	0.038 (0.009)***
factor(Country)Mexico	−0.019 (0.007)**
factor(Country)Netherlands	0.051 (0.009)***
factor(Country)Norway	0.064 (0.011)***
factor(Country)Philippines	−0.039 (0.007)***
factor(Country)Portugal	0.018 (0.008)*
factor(Country)Singapore	0.042 (0.012)***
factor(Country)South Africa	−0.033 (0.011)**
factor(Country)Spain	0.031 (0.007)***
factor(Country)Sweden	0.047 (0.009)***
factor(Country)Switzerland	0.048 (0.008)***
factor(Country)Thailand	0.031 (0.012)**
factor(Country)Turkey	−0.074 (0.011)***
factor(Country)United Kingdom	0.034 (0.007)***
factor(Country)United States of America	0.040 (0.007)***
factor(Country)Venezuela	−0.011 (0.007)
R^2	0.641
Adj. R^2	0.615
Num. obs.	646

$^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$, $^{\cdot}p < 0.1$

Appendix C

Appendix for Chapter 2

Table C.1: Example of results for inter-city industry versus services for all variability measures (here City2 selected as Singapore)

City	City2	IntraCityQadj	InterCityQadj	IntraCityLMResidVar	InterCityLMResidVar	IntraCityMAD	InterCityMAD
Amsterdam	Singapore	0.06976	0.11268	0.00918	0.07280	0.04665	0.20280
Bogota	Singapore	0.09505	0.11743	0.23349	0.06806	0.05644	0.16351
Brussels	Singapore	0.10402	0.26107	0.02469	0.06948	0.08769	0.17356
Buenos Aires	Singapore	0.21851	0.49557	0.09916	0.21918	0.05728	0.50679
Caracas	Singapore	0.10465	0.21771	0.07522	0.34284	0.16087	0.78315
Chicago	Singapore	0.09753	0.14396	0.02491	0.05269	0.12155	0.31667
Copenhagen	Singapore	0.06211	0.23478	0.01973	0.05933	0.04882	0.18011
Dublin	Singapore	0.04369	0.27435	0.01342	0.04466	0.06846	0.18337
Geneva	Singapore	0.04423	0.18419	0.02035	0.07310	0.08072	0.19731
Helsinki	Singapore	0.04329	0.21644	0.01048	0.07083	0.03243	0.15681
Hong Kong	Singapore	0.17861	0.21919	0.03878	0.07097	0.12778	0.13773
Johannesburg	Singapore	0.12233	0.15540	0.03442	0.03864	0.19322	0.20223
Lisbon	Singapore	0.16250	0.28803	0.07078	0.07672	0.05538	0.16349
London	Singapore	0.08854	0.11917	0.01288	0.04249	0.08343	0.13133
Los Angeles	Singapore	0.06769	0.16007	0.00646	0.02674	0.05244	0.21736
Luxembourg	Singapore	0.06327	0.17580	0.02899	0.06568	0.07611	0.16305
Madrid	Singapore	0.13659	0.36632	0.08765	0.07437	0.10614	0.18700
Manila	Singapore	0.11164	0.16683	0.02505	0.06952	0.10817	0.23474
Mexico City	Singapore	0.15219	0.30500	0.05244	0.27545	0.07923	0.48918
Milan	Singapore	0.06436	0.31536	0.04024	0.05838	0.02558	0.21461
Montreal	Singapore	0.06316	0.10359	0.00473	0.03347	0.07622	0.26507
New York	Singapore	0.05350	0.14424	0.01869	0.02646	0.04437	0.15590
Oslo	Singapore	0.08281	0.15208	0.01017	0.04495	0.08342	0.09373
Paris	Singapore	0.12202	0.09497	0.02438	0.04615	0.09825	0.15007
Rio de Janeiro	Singapore	0.29091	0.27938	0.10086	0.15309	0.05485	0.46372
Sao Paulo	Singapore	0.18643	0.29742	0.12288	0.22443	0.07223	0.53604
Stockholm	Singapore	0.03923	0.17733	0.01452	0.05359	0.06135	0.20919
Sydney	Singapore	0.08343	0.16085	0.00886	0.05925	0.05604	0.29782
Tel Aviv	Singapore	0.06696	0.16769	0.01794	0.03789	0.09975	0.12566
Tokyo	Singapore	0.04451	0.16465	0.03483	0.04923	0.06321	0.11814
Toronto	Singapore	0.10272	0.14776	0.00674	0.01486	0.05873	0.27163
Vienna	Singapore	0.04643	0.17838	0.03899	0.04395	0.05064	0.17247
Zurich	Singapore	0.01840	0.19622	0.01522	0.06686	0.04636	0.13243

Table C.2: Overview of results: Intra- and inter-country wage variability comparison. Composition of wage indices held constant over time, i.e. professions added to the survey after 1970 are excluded. With this limited data the indices reflecting the competitive versus uncompetitive sectors are not reported as insufficient professions are available to usefully construct these indices.

Wage variability compared	# of comparisons	# of $V(w_{ij}) < V(w_{ii^*})$			% of $V(w_{ij}) < V(w_{ii^*})$		
		ResVar	MAD	Qadj	ResVar	MAD	Qadj
All wage comparisons	4000	3779	3663	3502	94.5%	91.6%	87.6%
Inter-country industry, vs services	1000	971	937	884	97.1%	93.7%	88.4%
Inter-country services, vs industry	1000	914	895	859	91.4%	89.5%	85.9%
Inter-country skilled vs unskilled	1000	929	905	873	92.9%	90.5%	87.3%
Inter-country unskilled vs skilled	1000	965	926	886	96.5%	92.6%	88.6%

Table C.3: Overview of results: Intra- and inter-country wage variability comparison. 1976-2009: Composition of wage indices held constant over time, i.e. professions added to the survey after 1976 are excluded.

Wage variability compared	# of comparisons	# of $V(w_{ij}) < V(w_{ii^*})$			% of $V(w_{ij}) < V(w_{ii^*})$		
		ResVar	MAD	Qadj	ResVar	MAD	Qadj
All wage comparisons	6000	5690	5507	5150	94.8%	91.8 %	85.8%
Inter-country industry, vs services	1000	977	927	859	97.7%	92.7%	85.9%
Inter-country services, vs industry	1000	933	924	864	93.3%	92.4%	86.4%
Inter-country competitive vs uncompetitive	1000	985	927	899	98.5%	92.7%	89.9%
Inter-country uncompetitive vs competitive	1000	877	891	822	87.7%	89.1%	82.2%
Inter-country skilled vs unskilled	1000	966	907	860	96.6%	90.7%	86.0%
Inter-country unskilled vs skilled	1000	952	931	846	95.2%	93.1%	84.6%

Table C.4: Dixon and Mood test based on the variance of the regression residuals, for inter-country industry, vs services

	City	# of pairs with $V(w_{ij})$ larger	# of pairs with $V(w_{ii^*})$ larger	Dixon & Mood test statistic	H0	H0 (Bonferroni)
1	Amsterdam	1	22	4.17	Reject	Reject
2	Bogota	31	2	NA	NA	NA
3	Brussels	2	21	3.75	Reject	Reject
4	Buenos Aires	0	33	5.57	Reject	Reject
5	Caracas	0	33	5.57	Reject	Reject
6	Chicago	2	29	4.67	Reject	Reject
7	Copenhagen	4	19	2.92	Reject	Do not reject
8	Dublin	3	20	3.34	Reject	Reject
9	Geneva	7	25	3.01	Reject	Reject
10	Helsinki	0	23	4.59	Reject	Reject
11	Hong Kong	2	31	4.87	Reject	Reject
12	Johannesburg	5	28	3.83	Reject	Reject
13	Lisbon	3	20	3.34	Reject	Reject
14	London	3	30	4.53	Reject	Reject
15	Los Angeles	0	31	5.39	Reject	Reject
16	Luxembourg	2	21	3.75	Reject	Reject
17	Madrid	12	11	NA	NA	NA
18	Manila	0	33	5.57	Reject	Reject
19	Mexico City	0	33	5.57	Reject	Reject
20	Milan	2	21	3.75	Reject	Reject
21	Montreal	0	32	5.48	Reject	Reject
22	New York	2	29	4.67	Reject	Reject
23	Oslo	1	32	5.22	Reject	Reject
24	Paris	5	18	2.50	Reject	Do not reject
25	Rio de Janeiro	3	29	4.42	Reject	Reject
26	Sao Paulo	1	31	5.13	Reject	Reject
27	Singapore	3	30	4.53	Reject	Reject
28	Stockholm	1	32	5.22	Reject	Reject
29	Sydney	0	33	5.57	Reject	Reject
30	Tel Aviv	0	33	5.57	Reject	Reject
31	Tokyo	5	28	3.83	Reject	Reject
32	Toronto	0	32	5.48	Reject	Reject
33	Vienna	6	17	2.09	Reject	Do not reject
34	Zurich	8	24	2.65	Reject	Do not reject

Figure C.1: Distribution of ratios for inter-city industry vs services

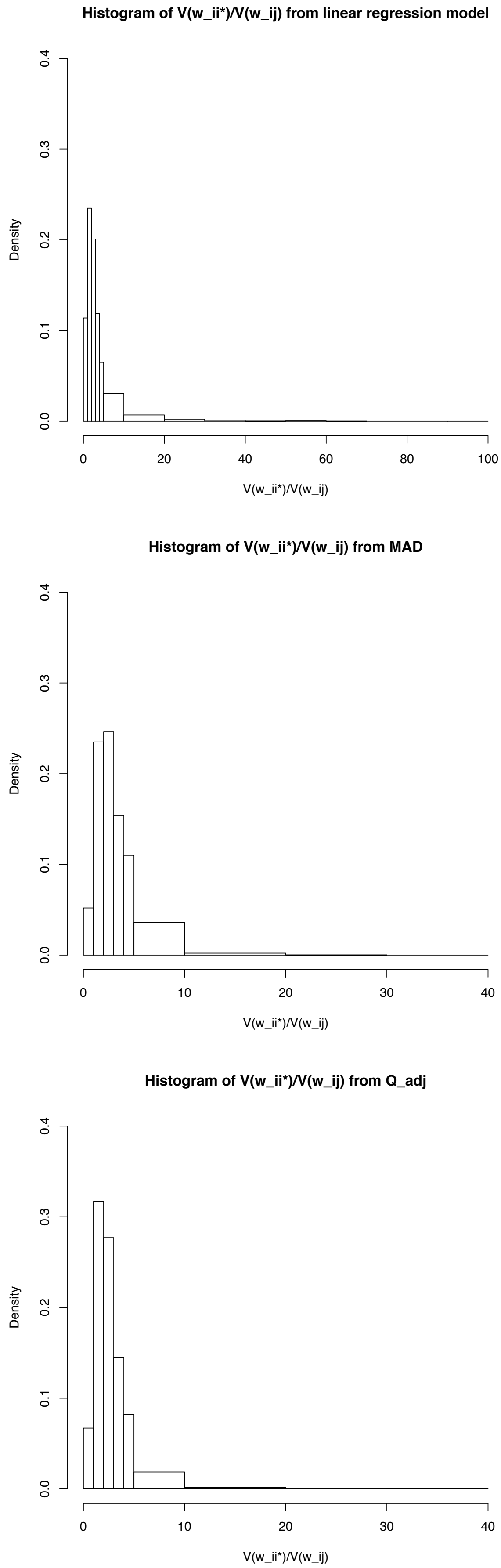


Figure C.2: Distribution of ratios for inter-city services vs industry

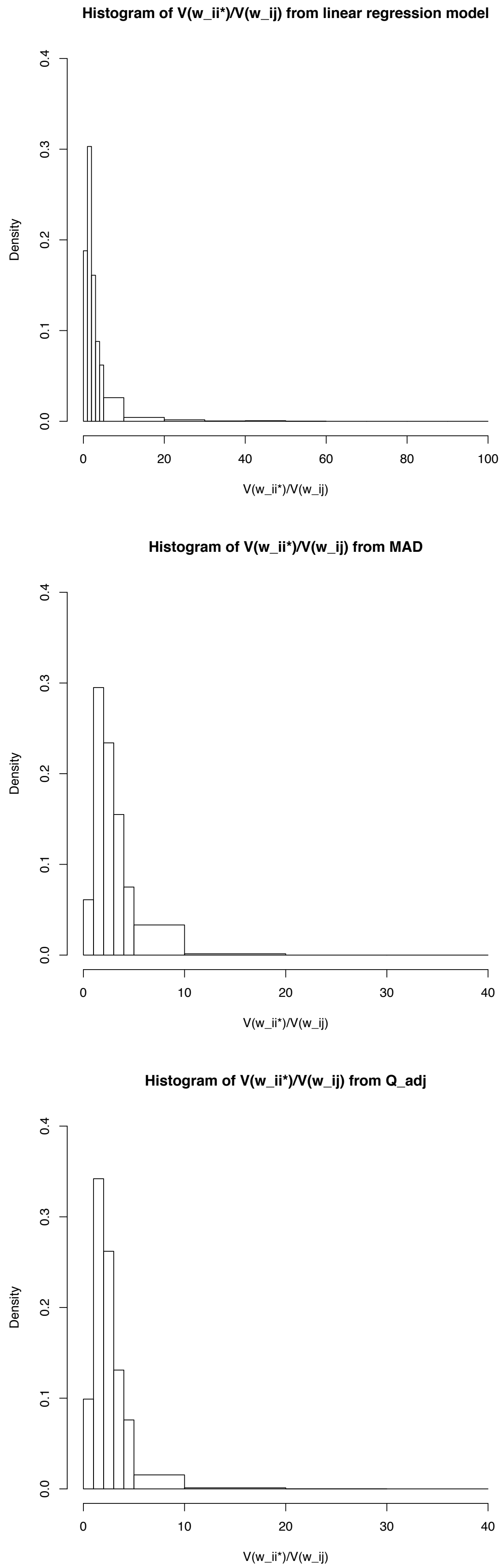
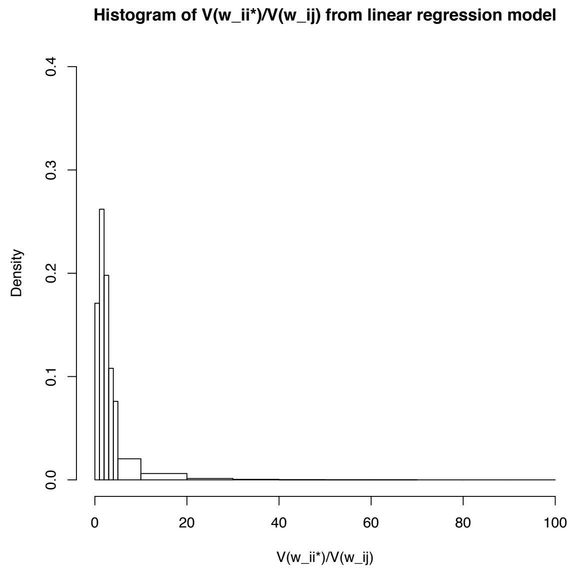
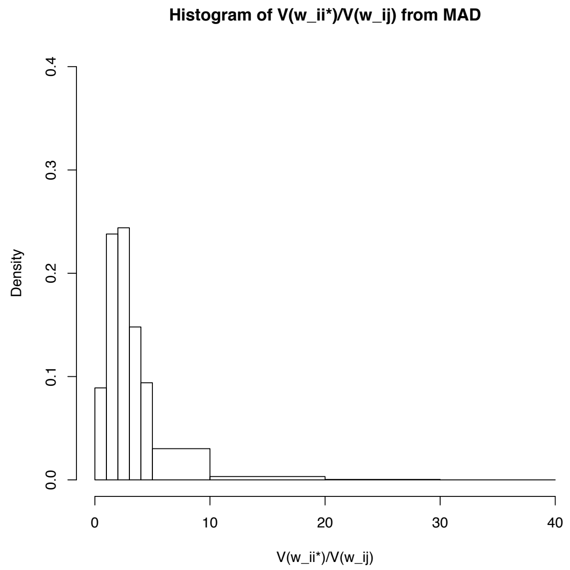


Figure C.3: Distribution of ratios for inter-city competitive vs uncompetitive





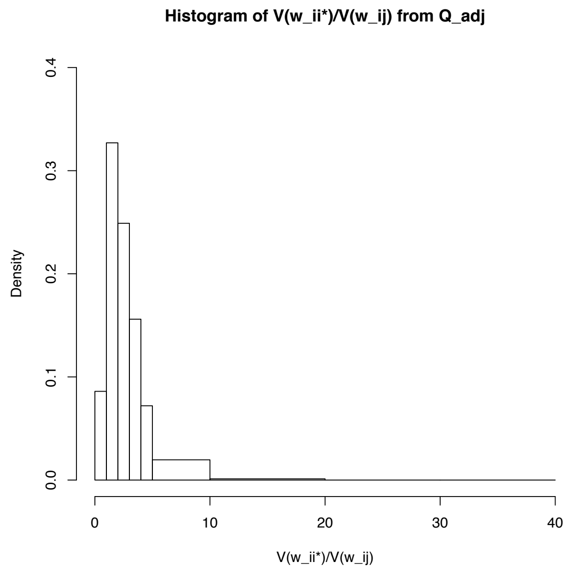


Figure C.4: Distribution of ratios for inter-city uncompetitive vs competitive

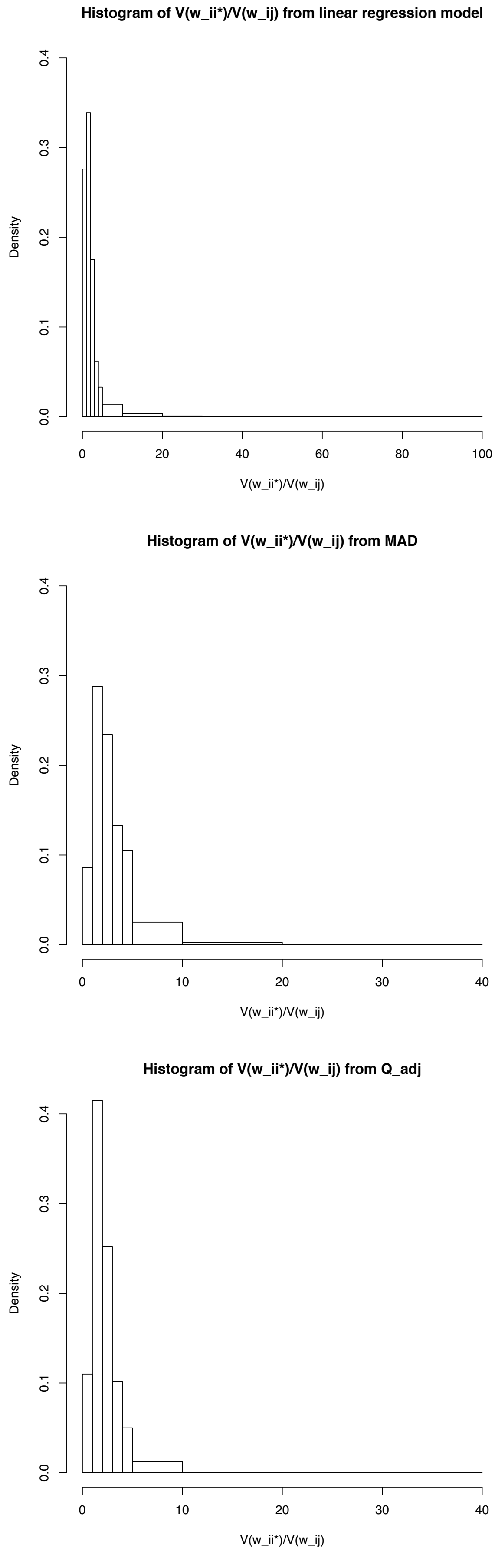
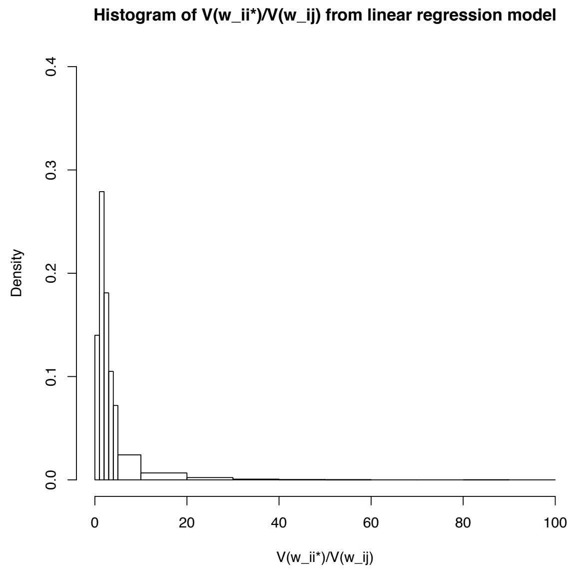
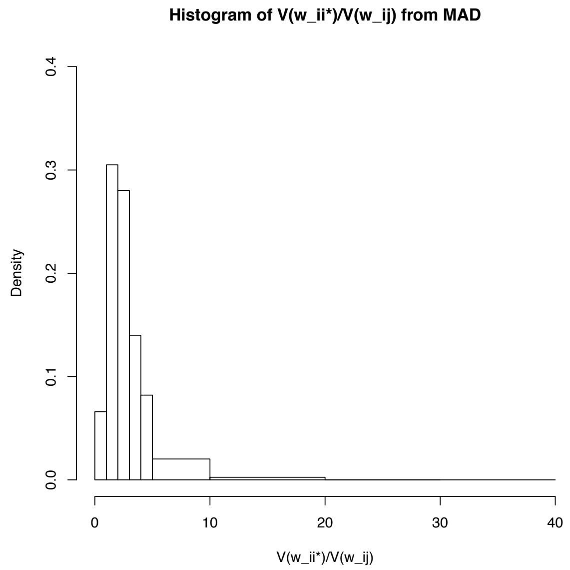


Figure C.5: Distribution of ratios for inter-city skilled vs unskilled





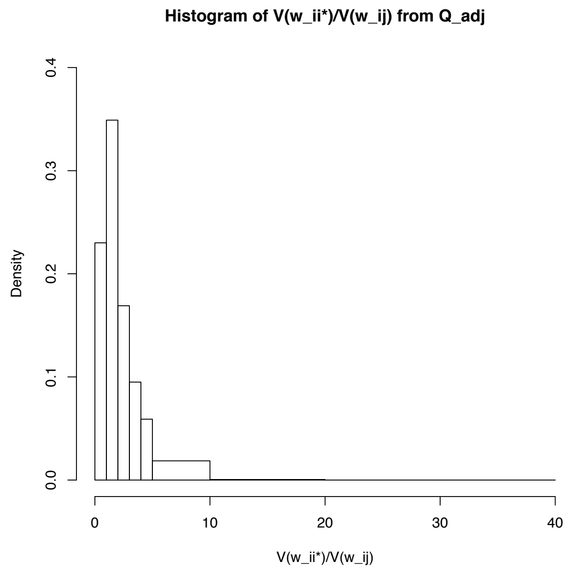


Figure C.6: Distribution of ratios for inter-city unskilled vs skilled

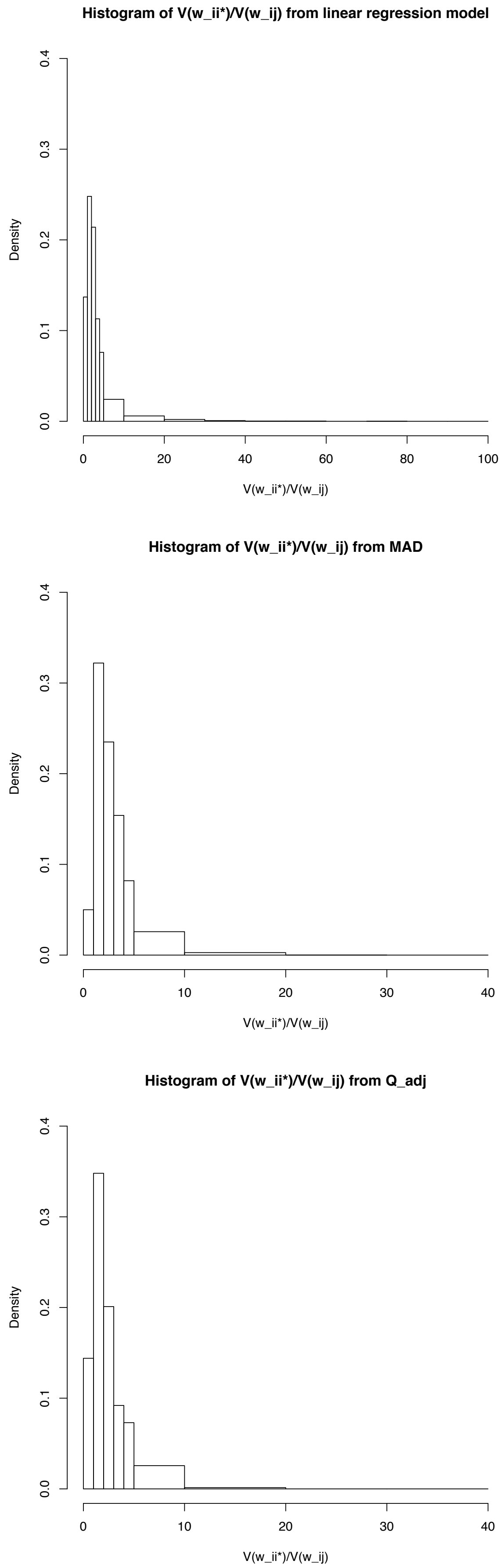


Table C.5: Dixon and Mood test based on the variance of the regression residuals, for inter-country services, vs industry

	City	# of pairs with $V(w_{ij})$ larger	# of pairs with $V(w_{ii^*})$ larger	Dixon & Mood test statistic	H0	H0 (Bonferroni)
1	Amsterdam	0	23	4.59	Reject	Reject
2	Bogota	32	1	NA	NA	NA
3	Brussels	4	19	2.92	Reject	Do not reject
4	Buenos Aires	0	33	5.57	Reject	Reject
5	Caracas	0	33	5.57	Reject	Reject
6	Chicago	6	25	3.23	Reject	Reject
7	Copenhagen	5	18	2.50	Reject	Do not reject
8	Dublin	3	20	3.34	Reject	Reject
9	Geneva	9	23	2.30	Reject	Do not reject
10	Helsinki	0	23	4.59	Reject	Reject
11	Hong Kong	17	16	NA	NA	NA
12	Johannesburg	10	23	2.09	Reject	Do not reject
13	Lisbon	7	16	1.67	Reject	Do not reject
14	London	0	33	5.57	Reject	Reject
15	Los Angeles	0	31	5.39	Reject	Reject
16	Luxembourg	8	15	1.25	Do not reject	Do not reject
17	Madrid	18	5	NA	NA	NA
18	Manila	3	30	4.53	Reject	Reject
19	Mexico City	0	33	5.57	Reject	Reject
20	Milan	9	14	0.83	Do not reject	Do not reject
21	Montreal	0	32	5.48	Reject	Reject
22	New York	3	28	4.31	Reject	Reject
23	Oslo	1	32	5.22	Reject	Reject
24	Paris	5	18	2.50	Reject	Do not reject
25	Rio de Janeiro	1	31	5.13	Reject	Reject
26	Sao Paulo	15	17	0.18	Do not reject	Do not reject
27	Singapore	11	22	1.74	Reject	Do not reject
28	Stockholm	2	31	4.87	Reject	Reject
29	Sydney	0	33	5.57	Reject	Reject
30	Tel Aviv	0	33	5.57	Reject	Reject
31	Tokyo	3	30	4.53	Reject	Reject
32	Toronto	0	32	5.48	Reject	Reject
33	Vienna	13	10	NA	NA	NA
34	Zurich	3	29	4.42	Reject	Reject

Table C.6: Dixon and Mood test based on the variance of the regression residuals, for inter-country competitive, vs uncompetitive

	City	# of pairs with $V(w_{ij})$ larger	# of pairs with $V(w_{ii^*})$ larger	Dixon & Mood test statistic	H0	H0 (Bonferroni)
1	Amsterdam	1	22	4.17	Reject	Reject
2	Bogota	31	2	NA	NA	NA
3	Brussels	6	17	2.09	Reject	Do not reject
4	Buenos Aires	0	33	5.57	Reject	Reject
5	Caracas	0	33	5.57	Reject	Reject
6	Chicago	2	29	4.67	Reject	Reject
7	Copenhagen	8	15	1.25	Do not reject	Do not reject
8	Dublin	5	18	2.50	Reject	Do not reject
9	Geneva	6	26	3.36	Reject	Reject
10	Helsinki	3	20	3.34	Reject	Reject
11	Hong Kong	1	32	5.22	Reject	Reject
12	Johannesburg	8	25	2.79	Reject	Do not reject
13	Lisbon	3	20	3.34	Reject	Reject
14	London	6	27	3.48	Reject	Reject
15	Los Angeles	0	31	5.39	Reject	Reject
16	Luxembourg	1	22	4.17	Reject	Reject
17	Madrid	16	7	NA	NA	NA
18	Manila	0	33	5.57	Reject	Reject
19	Mexico City	0	33	5.57	Reject	Reject
20	Milan	0	23	4.59	Reject	Reject
21	Montreal	1	31	5.13	Reject	Reject
22	New York	14	17	0.36	Do not reject	Do not reject
23	Oslo	19	14	NA	NA	NA
24	Paris	2	21	3.75	Reject	Reject
25	Rio de Janeiro	1	31	5.13	Reject	Reject
26	Sao Paulo	0	32	5.48	Reject	Reject
27	Singapore	7	26	3.13	Reject	Reject
28	Stockholm	8	25	2.79	Reject	Do not reject
29	Sydney	0	33	5.57	Reject	Reject
30	Tel Aviv	0	33	5.57	Reject	Reject
31	Tokyo	4	29	4.18	Reject	Reject
32	Toronto	0	32	5.48	Reject	Reject
33	Vienna	13	10	NA	NA	NA
34	Zurich	5	27	3.71	Reject	Reject

Table C.7: Dixon and Mood test based on the variance of the regression residuals, for inter-country uncompetitive, vs competitive

	City	# of pairs with $V(w_{ij})$ larger	# of pairs with $V(w_{ii^*})$ larger	Dixon & Mood test statistic	H0	H0 (Bonferroni)
1	Amsterdam	0	23	4.59	Reject	Reject
2	Bogota	33	0	NA	NA	NA
3	Brussels	14	9	NA	NA	NA
4	Buenos Aires	0	33	5.57	Reject	Reject
5	Caracas	0	33	5.57	Reject	Reject
6	Chicago	21	10	NA	NA	NA
7	Copenhagen	10	13	0.42	Do not reject	Do not reject
8	Dublin	6	17	2.09	Reject	Do not reject
9	Geneva	9	23	2.30	Reject	Do not reject
10	Helsinki	0	23	4.59	Reject	Reject
11	Hong Kong	2	31	4.87	Reject	Reject
12	Johannesburg	13	20	1.04	Do not reject	Do not reject
13	Lisbon	9	14	0.83	Do not reject	Do not reject
14	London	2	31	4.87	Reject	Reject
15	Los Angeles	0	31	5.39	Reject	Reject
16	Luxembourg	13	10	NA	NA	NA
17	Madrid	19	4	NA	NA	NA
18	Manila	10	23	2.09	Reject	Do not reject
19	Mexico City	0	33	5.57	Reject	Reject
20	Milan	7	16	1.67	Reject	Do not reject
21	Montreal	3	29	4.42	Reject	Reject
22	New York	21	10	NA	NA	NA
23	Oslo	23	10	NA	NA	NA
24	Paris	5	18	2.50	Reject	Do not reject
25	Rio de Janeiro	0	32	5.48	Reject	Reject
26	Sao Paulo	0	32	5.48	Reject	Reject
27	Singapore	14	19	0.70	Do not reject	Do not reject
28	Stockholm	17	16	NA	NA	NA
29	Sydney	1	32	5.22	Reject	Reject
30	Tel Aviv	1	32	5.22	Reject	Reject
31	Tokyo	0	33	5.57	Reject	Reject
32	Toronto	1	31	5.13	Reject	Reject
33	Vienna	15	8	NA	NA	NA
34	Zurich	7	25	3.01	Reject	Reject

Table C.8: Dixon and Mood test based on the variance of the regression residuals, for inter-country skilled, vs unskilled

	City	# of pairs with $V(w_{ij})$ larger	# of pairs with $V(w_{ii^*})$ larger	Dixon & Mood test statistic	H0	H0 (Bonferroni)
1	Amsterdam	2	21	3.75	Reject	Reject
2	Bogota	21	12	NA	NA	NA
3	Brussels	2	21	3.75	Reject	Reject
4	Buenos Aires	0	33	5.57	Reject	Reject
5	Caracas	0	33	5.57	Reject	Reject
6	Chicago	7	24	2.87	Reject	Do not reject
7	Copenhagen	0	23	4.59	Reject	Reject
8	Dublin	0	23	4.59	Reject	Reject
9	Geneva	9	23	2.30	Reject	Do not reject
10	Helsinki	2	21	3.75	Reject	Reject
11	Hong Kong	23	10	NA	NA	NA
12	Johannesburg	16	17	0.00	Do not reject	Do not reject
13	Lisbon	0	23	4.59	Reject	Reject
14	London	4	29	4.18	Reject	Reject
15	Los Angeles	1	30	5.03	Reject	Reject
16	Luxembourg	6	17	2.09	Reject	Do not reject
17	Madrid	0	23	4.59	Reject	Reject
18	Manila	0	33	5.57	Reject	Reject
19	Mexico City	0	33	5.57	Reject	Reject
20	Milan	0	23	4.59	Reject	Reject
21	Montreal	6	26	3.36	Reject	Reject
22	New York	6	25	3.23	Reject	Reject
23	Oslo	0	33	5.57	Reject	Reject
24	Paris	6	17	2.09	Reject	Do not reject
25	Rio de Janeiro	2	30	4.77	Reject	Reject
26	Sao Paulo	1	31	5.13	Reject	Reject
27	Singapore	1	32	5.22	Reject	Reject
28	Stockholm	0	33	5.57	Reject	Reject
29	Sydney	0	33	5.57	Reject	Reject
30	Tel Aviv	15	18	0.35	Do not reject	Do not reject
31	Tokyo	0	33	5.57	Reject	Reject
32	Toronto	2	30	4.77	Reject	Reject
33	Vienna	2	21	3.75	Reject	Reject
34	Zurich	6	26	3.36	Reject	Reject

Table C.9: Dixon and Mood test based on the variance of the regression residuals, for inter-country unskilled, vs skilled

	City	# of pairs with $V(w_{ij})$ larger	# of pairs with $V(w_{ii^*})$ larger	Dixon & Mood test statistic	H0	H0 (Bonferroni)
1	Amsterdam	3	20	3.34	Reject	Reject
2	Bogota	13	20	1.04	Do not reject	Do not reject
3	Brussels	3	20	3.34	Reject	Reject
4	Buenos Aires	0	33	5.57	Reject	Reject
5	Caracas	0	33	5.57	Reject	Reject
6	Chicago	2	29	4.67	Reject	Reject
7	Copenhagen	2	21	3.75	Reject	Reject
8	Dublin	0	23	4.59	Reject	Reject
9	Geneva	9	23	2.30	Reject	Do not reject
10	Helsinki	5	18	2.50	Reject	Do not reject
11	Hong Kong	20	13	NA	NA	NA
12	Johannesburg	6	27	3.48	Reject	Reject
13	Lisbon	0	23	4.59	Reject	Reject
14	London	0	33	5.57	Reject	Reject
15	Los Angeles	3	28	4.31	Reject	Reject
16	Luxembourg	3	20	3.34	Reject	Reject
17	Madrid	7	16	1.67	Reject	Do not reject
18	Manila	4	29	4.18	Reject	Reject
19	Mexico City	0	33	5.57	Reject	Reject
20	Milan	0	23	4.59	Reject	Reject
21	Montreal	2	30	4.77	Reject	Reject
22	New York	4	27	3.95	Reject	Reject
23	Oslo	1	32	5.22	Reject	Reject
24	Paris	3	20	3.34	Reject	Reject
25	Rio de Janeiro	5	27	3.71	Reject	Reject
26	Sao Paulo	22	10	NA	NA	NA
27	Singapore	0	33	5.57	Reject	Reject
28	Stockholm	0	33	5.57	Reject	Reject
29	Sydney	0	33	5.57	Reject	Reject
30	Tel Aviv	9	24	2.44	Reject	Do not reject
31	Tokyo	0	33	5.57	Reject	Reject
32	Toronto	2	30	4.77	Reject	Reject
33	Vienna	2	21	3.75	Reject	Reject
34	Zurich	7	25	3.01	Reject	Reject

Table C.10: Dixon and Mood test based on MAD, for inter-country industry, vs services

	City	# of pairs with $V(w_{ij})$ larger	# of pairs with $V(w_{ii^*})$ larger	Dixon & Mood test statistic	H0	H0 (Bonferroni)
1	Amsterdam	1	22	4.17	Reject	Reject
2	Bogota	1	32	5.22	Reject	Reject
3	Brussels	2	21	3.75	Reject	Reject
4	Buenos Aires	0	33	5.57	Reject	Reject
5	Caracas	0	33	5.57	Reject	Reject
6	Chicago	5	26	3.59	Reject	Reject
7	Copenhagen	0	23	4.59	Reject	Reject
8	Dublin	0	23	4.59	Reject	Reject
9	Geneva	6	26	3.36	Reject	Reject
10	Helsinki	0	23	4.59	Reject	Reject
11	Hong Kong	5	28	3.83	Reject	Reject
12	Johannesburg	13	20	1.04	Do not reject	Do not reject
13	Lisbon	0	23	4.59	Reject	Reject
14	London	5	28	3.83	Reject	Reject
15	Los Angeles	0	31	5.39	Reject	Reject
16	Luxembourg	0	23	4.59	Reject	Reject
17	Madrid	1	22	4.17	Reject	Reject
18	Manila	0	33	5.57	Reject	Reject
19	Mexico City	0	33	5.57	Reject	Reject
20	Milan	0	23	4.59	Reject	Reject
21	Montreal	0	32	5.48	Reject	Reject
22	New York	1	30	5.03	Reject	Reject
23	Oslo	3	30	4.53	Reject	Reject
24	Paris	5	18	2.50	Reject	Do not reject
25	Rio de Janeiro	0	32	5.48	Reject	Reject
26	Sao Paulo	0	32	5.48	Reject	Reject
27	Singapore	0	33	5.57	Reject	Reject
28	Stockholm	1	32	5.22	Reject	Reject
29	Sydney	0	33	5.57	Reject	Reject
30	Tel Aviv	0	33	5.57	Reject	Reject
31	Tokyo	0	33	5.57	Reject	Reject
32	Toronto	0	32	5.48	Reject	Reject
33	Vienna	1	22	4.17	Reject	Reject
34	Zurich	2	30	4.77	Reject	Reject

Table C.11: Dixon and Mood test based on MAD, for inter-country services, vs industry

	City	# of pairs with $V(w_{ij})$ larger	# of pairs with $V(w_{ii^*})$ larger	Dixon & Mood test statistic	H0	H0 (Bonferroni)
1	Amsterdam	0	23	4.59	Reject	Reject
2	Bogota	0	33	5.57	Reject	Reject
3	Brussels	3	20	3.34	Reject	Reject
4	Buenos Aires	0	33	5.57	Reject	Reject
5	Caracas	0	33	5.57	Reject	Reject
6	Chicago	9	22	2.16	Reject	Do not reject
7	Copenhagen	0	23	4.59	Reject	Reject
8	Dublin	1	22	4.17	Reject	Reject
9	Geneva	5	27	3.71	Reject	Reject
10	Helsinki	0	23	4.59	Reject	Reject
11	Hong Kong	8	25	2.79	Reject	Do not reject
12	Johannesburg	5	28	3.83	Reject	Reject
13	Lisbon	0	23	4.59	Reject	Reject
14	London	7	26	3.13	Reject	Reject
15	Los Angeles	1	30	5.03	Reject	Reject
16	Luxembourg	0	23	4.59	Reject	Reject
17	Madrid	4	19	2.92	Reject	Do not reject
18	Manila	5	28	3.83	Reject	Reject
19	Mexico City	0	33	5.57	Reject	Reject
20	Milan	0	23	4.59	Reject	Reject
21	Montreal	2	30	4.77	Reject	Reject
22	New York	0	31	5.39	Reject	Reject
23	Oslo	3	30	4.53	Reject	Reject
24	Paris	2	21	3.75	Reject	Reject
25	Rio de Janeiro	0	32	5.48	Reject	Reject
26	Sao Paulo	0	32	5.48	Reject	Reject
27	Singapore	0	33	5.57	Reject	Reject
28	Stockholm	0	33	5.57	Reject	Reject
29	Sydney	1	32	5.22	Reject	Reject
30	Tel Aviv	2	31	4.87	Reject	Reject
31	Tokyo	0	33	5.57	Reject	Reject
32	Toronto	0	32	5.48	Reject	Reject
33	Vienna	2	21	3.75	Reject	Reject
34	Zurich	1	31	5.13	Reject	Reject

Table C.12: Dixon and Mood test based on MAD, for inter-country competitive, vs uncompetitive

	City	# of pairs with $V(w_{ij})$ larger	# of pairs with $V(w_{ii^*})$ larger	Dixon & Mood test statistic	H0	H0 (Bonferroni)
1	Amsterdam	6	17	2.09	Reject	Do not reject
2	Bogota	0	33	5.57	Reject	Reject
3	Brussels	8	15	1.25	Do not reject	Do not reject
4	Buenos Aires	0	33	5.57	Reject	Reject
5	Caracas	0	33	5.57	Reject	Reject
6	Chicago	5	26	3.59	Reject	Reject
7	Copenhagen	0	23	4.59	Reject	Reject
8	Dublin	1	22	4.17	Reject	Reject
9	Geneva	1	31	5.13	Reject	Reject
10	Helsinki	4	19	2.92	Reject	Do not reject
11	Hong Kong	6	27	3.48	Reject	Reject
12	Johannesburg	19	14	NA	NA	NA
13	Lisbon	0	23	4.59	Reject	Reject
14	London	1	32	5.22	Reject	Reject
15	Los Angeles	0	31	5.39	Reject	Reject
16	Luxembourg	0	23	4.59	Reject	Reject
17	Madrid	5	18	2.50	Reject	Do not reject
18	Manila	2	31	4.87	Reject	Reject
19	Mexico City	0	33	5.57	Reject	Reject
20	Milan	0	23	4.59	Reject	Reject
21	Montreal	0	32	5.48	Reject	Reject
22	New York	0	31	5.39	Reject	Reject
23	Oslo	14	19	0.70	Do not reject	Do not reject
24	Paris	14	9	NA	NA	NA
25	Rio de Janeiro	0	32	5.48	Reject	Reject
26	Sao Paulo	0	32	5.48	Reject	Reject
27	Singapore	0	33	5.57	Reject	Reject
28	Stockholm	1	32	5.22	Reject	Reject
29	Sydney	2	31	4.87	Reject	Reject
30	Tel Aviv	0	33	5.57	Reject	Reject
31	Tokyo	0	33	5.57	Reject	Reject
32	Toronto	0	32	5.48	Reject	Reject
33	Vienna	0	23	4.59	Reject	Reject
34	Zurich	0	32	5.48	Reject	Reject

Table C.13: Dixon and Mood test based on MAD, for inter-country uncompetitive, vs competitive

	City	# of pairs with $V(w_{ij})$ larger	# of pairs with $V(w_{ii^*})$ larger	Dixon & Mood test statistic	H0	H0 (Bonferroni)
1	Amsterdam	2	21	3.75	Reject	Reject
2	Bogota	0	33	5.57	Reject	Reject
3	Brussels	7	16	1.67	Reject	Do not reject
4	Buenos Aires	0	33	5.57	Reject	Reject
5	Caracas	0	33	5.57	Reject	Reject
6	Chicago	7	24	2.87	Reject	Do not reject
7	Copenhagen	0	23	4.59	Reject	Reject
8	Dublin	1	22	4.17	Reject	Reject
9	Geneva	3	29	4.42	Reject	Reject
10	Helsinki	1	22	4.17	Reject	Reject
11	Hong Kong	6	27	3.48	Reject	Reject
12	Johannesburg	3	30	4.53	Reject	Reject
13	Lisbon	2	21	3.75	Reject	Reject
14	London	0	33	5.57	Reject	Reject
15	Los Angeles	2	29	4.67	Reject	Reject
16	Luxembourg	3	20	3.34	Reject	Reject
17	Madrid	3	20	3.34	Reject	Reject
18	Manila	9	24	2.44	Reject	Do not reject
19	Mexico City	0	33	5.57	Reject	Reject
20	Milan	0	23	4.59	Reject	Reject
21	Montreal	2	30	4.77	Reject	Reject
22	New York	0	31	5.39	Reject	Reject
23	Oslo	17	16	NA	NA	NA
24	Paris	10	13	0.42	Do not reject	Do not reject
25	Rio de Janeiro	0	32	5.48	Reject	Reject
26	Sao Paulo	0	32	5.48	Reject	Reject
27	Singapore	2	31	4.87	Reject	Reject
28	Stockholm	1	32	5.22	Reject	Reject
29	Sydney	4	29	4.18	Reject	Reject
30	Tel Aviv	0	33	5.57	Reject	Reject
31	Tokyo	0	33	5.57	Reject	Reject
32	Toronto	0	32	5.48	Reject	Reject
33	Vienna	1	22	4.17	Reject	Reject
34	Zurich	0	32	5.48	Reject	Reject

Table C.14: Dixon and Mood test based on MAD, for inter-country skilled, vs unskilled

	City	# of pairs with $V(w_{ij})$ larger	# of pairs with $V(w_{ii^*})$ larger	Dixon & Mood test statistic	H0	H0 (Bonferroni)
1	Amsterdam	1	22	4.17	Reject	Reject
2	Bogota	4	29	4.18	Reject	Reject
3	Brussels	3	20	3.34	Reject	Reject
4	Buenos Aires	0	33	5.57	Reject	Reject
5	Caracas	0	33	5.57	Reject	Reject
6	Chicago	3	28	4.31	Reject	Reject
7	Copenhagen	0	23	4.59	Reject	Reject
8	Dublin	0	23	4.59	Reject	Reject
9	Geneva	3	29	4.42	Reject	Reject
10	Helsinki	0	23	4.59	Reject	Reject
11	Hong Kong	8	25	2.79	Reject	Do not reject
12	Johannesburg	2	31	4.87	Reject	Reject
13	Lisbon	0	23	4.59	Reject	Reject
14	London	6	27	3.48	Reject	Reject
15	Los Angeles	0	31	5.39	Reject	Reject
16	Luxembourg	0	23	4.59	Reject	Reject
17	Madrid	0	23	4.59	Reject	Reject
18	Manila	0	33	5.57	Reject	Reject
19	Mexico City	0	33	5.57	Reject	Reject
20	Milan	0	23	4.59	Reject	Reject
21	Montreal	8	24	2.65	Reject	Do not reject
22	New York	1	30	5.03	Reject	Reject
23	Oslo	2	31	4.87	Reject	Reject
24	Paris	2	21	3.75	Reject	Reject
25	Rio de Janeiro	1	31	5.13	Reject	Reject
26	Sao Paulo	0	32	5.48	Reject	Reject
27	Singapore	4	29	4.18	Reject	Reject
28	Stockholm	1	32	5.22	Reject	Reject
29	Sydney	0	33	5.57	Reject	Reject
30	Tel Aviv	6	27	3.48	Reject	Reject
31	Tokyo	1	32	5.22	Reject	Reject
32	Toronto	7	25	3.01	Reject	Reject
33	Vienna	1	22	4.17	Reject	Reject
34	Zurich	2	30	4.77	Reject	Reject

Table C.15: Dixon and Mood test based on MAD, for inter-country unskilled, vs skilled

	City	# of pairs with $V(w_{ij})$ larger	# of pairs with $V(w_{ii^*})$ larger	Dixon & Mood test statistic	H0	H0 (Bonferroni)
1	Amsterdam	0	23	4.59	Reject	Reject
2	Bogota	4	29	4.18	Reject	Reject
3	Brussels	3	20	3.34	Reject	Reject
4	Buenos Aires	0	33	5.57	Reject	Reject
5	Caracas	0	33	5.57	Reject	Reject
6	Chicago	2	29	4.67	Reject	Reject
7	Copenhagen	0	23	4.59	Reject	Reject
8	Dublin	0	23	4.59	Reject	Reject
9	Geneva	3	29	4.42	Reject	Reject
10	Helsinki	0	23	4.59	Reject	Reject
11	Hong Kong	1	32	5.22	Reject	Reject
12	Johannesburg	2	31	4.87	Reject	Reject
13	Lisbon	0	23	4.59	Reject	Reject
14	London	2	31	4.87	Reject	Reject
15	Los Angeles	0	31	5.39	Reject	Reject
16	Luxembourg	0	23	4.59	Reject	Reject
17	Madrid	1	22	4.17	Reject	Reject
18	Manila	3	30	4.53	Reject	Reject
19	Mexico City	0	33	5.57	Reject	Reject
20	Milan	0	23	4.59	Reject	Reject
21	Montreal	7	25	3.01	Reject	Reject
22	New York	1	30	5.03	Reject	Reject
23	Oslo	2	31	4.87	Reject	Reject
24	Paris	3	20	3.34	Reject	Reject
25	Rio de Janeiro	3	29	4.42	Reject	Reject
26	Sao Paulo	3	29	4.42	Reject	Reject
27	Singapore	2	31	4.87	Reject	Reject
28	Stockholm	0	33	5.57	Reject	Reject
29	Sydney	0	33	5.57	Reject	Reject
30	Tel Aviv	1	32	5.22	Reject	Reject
31	Tokyo	0	33	5.57	Reject	Reject
32	Toronto	3	29	4.42	Reject	Reject
33	Vienna	1	22	4.17	Reject	Reject
34	Zurich	3	29	4.42	Reject	Reject

Table C.16: Dixon and Mood test based on Q^a_{adj}, for inter-country industry, vs services

	City	# of pairs with $V(w_{ij})$ larger	# of pairs with $V(w_{ii^*})$ larger	Dixon & Mood test statistic	H0	H0 (Bonferroni)
1	Amsterdam	1	22	4.17	Reject	Reject
2	Bogota	1	32	5.22	Reject	Reject
3	Brussels	3	20	3.34	Reject	Reject
4	Buenos Aires	1	32	5.22	Reject	Reject
5	Caracas	0	33	5.57	Reject	Reject
6	Chicago	1	30	5.03	Reject	Reject
7	Copenhagen	1	22	4.17	Reject	Reject
8	Dublin	0	23	4.59	Reject	Reject
9	Geneva	1	31	5.13	Reject	Reject
10	Helsinki	1	22	4.17	Reject	Reject
11	Hong Kong	6	27	3.48	Reject	Reject
12	Johannesburg	2	31	4.87	Reject	Reject
13	Lisbon	3	20	3.34	Reject	Reject
14	London	5	28	3.83	Reject	Reject
15	Los Angeles	1	30	5.03	Reject	Reject
16	Luxembourg	1	22	4.17	Reject	Reject
17	Madrid	1	22	4.17	Reject	Reject
18	Manila	2	31	4.87	Reject	Reject
19	Mexico City	2	31	4.87	Reject	Reject
20	Milan	0	23	4.59	Reject	Reject
21	Montreal	0	32	5.48	Reject	Reject
22	New York	0	31	5.39	Reject	Reject
23	Oslo	5	28	3.83	Reject	Reject
24	Paris	10	13	0.42	Do not reject	Do not reject
25	Rio de Janeiro	9	23	2.30	Reject	Do not reject
26	Sao Paulo	2	30	4.77	Reject	Reject
27	Singapore	0	33	5.57	Reject	Reject
28	Stockholm	0	33	5.57	Reject	Reject
29	Sydney	1	32	5.22	Reject	Reject
30	Tel Aviv	0	33	5.57	Reject	Reject
31	Tokyo	0	33	5.57	Reject	Reject
32	Toronto	5	27	3.71	Reject	Reject
33	Vienna	2	21	3.75	Reject	Reject
34	Zurich	0	32	5.48	Reject	Reject

Table C.17: Dixon and Mood test based on Q^a_{adj}, for inter-country services, vs industry

	City	# of pairs with $V(w_{ij})$ larger	# of pairs with $V(w_{ii^*})$ larger	Dixon & Mood test statistic	H0	H0 (Bonferroni)
1	Amsterdam	2	21	3.75	Reject	Reject
2	Bogota	1	32	5.22	Reject	Reject
3	Brussels	5	18	2.50	Reject	Do not reject
4	Buenos Aires	0	33	5.57	Reject	Reject
5	Caracas	0	33	5.57	Reject	Reject
6	Chicago	5	26	3.59	Reject	Reject
7	Copenhagen	3	20	3.34	Reject	Reject
8	Dublin	0	23	4.59	Reject	Reject
9	Geneva	0	32	5.48	Reject	Reject
10	Helsinki	1	22	4.17	Reject	Reject
11	Hong Kong	20	13	NA	NA	NA
12	Johannesburg	4	29	4.18	Reject	Reject
13	Lisbon	13	10	NA	NA	NA
14	London	1	32	5.22	Reject	Reject
15	Los Angeles	0	31	5.39	Reject	Reject
16	Luxembourg	1	22	4.17	Reject	Reject
17	Madrid	5	18	2.50	Reject	Do not reject
18	Manila	0	33	5.57	Reject	Reject
19	Mexico City	1	32	5.22	Reject	Reject
20	Milan	1	22	4.17	Reject	Reject
21	Montreal	1	31	5.13	Reject	Reject
22	New York	0	31	5.39	Reject	Reject
23	Oslo	6	27	3.48	Reject	Reject
24	Paris	4	19	2.92	Reject	Do not reject
25	Rio de Janeiro	20	12	NA	NA	NA
26	Sao Paulo	2	30	4.77	Reject	Reject
27	Singapore	0	33	5.57	Reject	Reject
28	Stockholm	1	32	5.22	Reject	Reject
29	Sydney	1	32	5.22	Reject	Reject
30	Tel Aviv	0	33	5.57	Reject	Reject
31	Tokyo	0	33	5.57	Reject	Reject
32	Toronto	0	32	5.48	Reject	Reject
33	Vienna	1	22	4.17	Reject	Reject
34	Zurich	0	32	5.48	Reject	Reject

Table C.18: Dixon and Mood test based on Q^a_{adj}, for inter-country competitive, vs uncompetitive

	City	# of pairs with $V(w_{ij})$ larger	# of pairs with $V(w_{ii^*})$ larger	Dixon & Mood test statistic	H0	H0 (Bonferroni)
1	Amsterdam	0	23	4.59	Reject	Reject
2	Bogota	2	31	4.87	Reject	Reject
3	Brussels	1	22	4.17	Reject	Reject
4	Buenos Aires	0	33	5.57	Reject	Reject
5	Caracas	10	23	2.09	Reject	Do not reject
6	Chicago	0	31	5.39	Reject	Reject
7	Copenhagen	1	22	4.17	Reject	Reject
8	Dublin	0	23	4.59	Reject	Reject
9	Geneva	0	32	5.48	Reject	Reject
10	Helsinki	2	21	3.75	Reject	Reject
11	Hong Kong	1	32	5.22	Reject	Reject
12	Johannesburg	2	31	4.87	Reject	Reject
13	Lisbon	8	15	1.25	Do not reject	Do not reject
14	London	2	31	4.87	Reject	Reject
15	Los Angeles	0	31	5.39	Reject	Reject
16	Luxembourg	0	23	4.59	Reject	Reject
17	Madrid	3	20	3.34	Reject	Reject
18	Manila	5	28	3.83	Reject	Reject
19	Mexico City	0	33	5.57	Reject	Reject
20	Milan	1	22	4.17	Reject	Reject
21	Montreal	7	25	3.01	Reject	Reject
22	New York	0	31	5.39	Reject	Reject
23	Oslo	16	17	0.00	Do not reject	Do not reject
24	Paris	8	15	1.25	Do not reject	Do not reject
25	Rio de Janeiro	2	30	4.77	Reject	Reject
26	Sao Paulo	8	24	2.65	Reject	Do not reject
27	Singapore	1	32	5.22	Reject	Reject
28	Stockholm	0	33	5.57	Reject	Reject
29	Sydney	0	33	5.57	Reject	Reject
30	Tel Aviv	2	31	4.87	Reject	Reject
31	Tokyo	0	33	5.57	Reject	Reject
32	Toronto	4	28	4.07	Reject	Reject
33	Vienna	0	23	4.59	Reject	Reject
34	Zurich	0	32	5.48	Reject	Reject

Table C.19: Dixon and Mood test based on Q^a_{adj}, for inter-country uncompetitive, vs competitive

	City	# of pairs with $V(w_{ij})$ larger	# of pairs with $V(w_{ii^*})$ larger	Dixon & Mood test statistic	H0	H0 (Bonferroni)
1	Amsterdam	2	21	3.75	Reject	Reject
2	Bogota	5	28	3.83	Reject	Reject
3	Brussels	4	19	2.92	Reject	Do not reject
4	Buenos Aires	0	33	5.57	Reject	Reject
5	Caracas	13	20	1.04	Do not reject	Do not reject
6	Chicago	0	31	5.39	Reject	Reject
7	Copenhagen	2	21	3.75	Reject	Reject
8	Dublin	0	23	4.59	Reject	Reject
9	Geneva	3	29	4.42	Reject	Reject
10	Helsinki	4	19	2.92	Reject	Do not reject
11	Hong Kong	1	32	5.22	Reject	Reject
12	Johannesburg	2	31	4.87	Reject	Reject
13	Lisbon	18	5	NA	NA	NA
14	London	0	33	5.57	Reject	Reject
15	Los Angeles	0	31	5.39	Reject	Reject
16	Luxembourg	1	22	4.17	Reject	Reject
17	Madrid	7	16	1.67	Reject	Do not reject
18	Manila	3	30	4.53	Reject	Reject
19	Mexico City	0	33	5.57	Reject	Reject
20	Milan	1	22	4.17	Reject	Reject
21	Montreal	4	28	4.07	Reject	Reject
22	New York	0	31	5.39	Reject	Reject
23	Oslo	22	11	NA	NA	NA
24	Paris	1	22	4.17	Reject	Reject
25	Rio de Janeiro	3	29	4.42	Reject	Reject
26	Sao Paulo	8	24	2.65	Reject	Do not reject
27	Singapore	0	33	5.57	Reject	Reject
28	Stockholm	0	33	5.57	Reject	Reject
29	Sydney	1	32	5.22	Reject	Reject
30	Tel Aviv	1	32	5.22	Reject	Reject
31	Tokyo	0	33	5.57	Reject	Reject
32	Toronto	3	29	4.42	Reject	Reject
33	Vienna	1	22	4.17	Reject	Reject
34	Zurich	0	32	5.48	Reject	Reject

Table C.20: Dixon and Mood test based on Q^a_{adj}, for inter-country skilled vs unskilled

	City	# of pairs with $V(w_{ij})$ larger	# of pairs with $V(w_{ii^*})$ larger	Dixon & Mood test statistic	H0	H0 (Bonferroni)
1	Amsterdam	0	23	4.59	Reject	Reject
2	Bogota	13	20	1.04	Do not reject	Do not reject
3	Brussels	3	20	3.34	Reject	Reject
4	Buenos Aires	1	32	5.22	Reject	Reject
5	Caracas	30	3	NA	NA	NA
6	Chicago	24	7	NA	NA	NA
7	Copenhagen	2	21	3.75	Reject	Reject
8	Dublin	0	23	4.59	Reject	Reject
9	Geneva	1	31	5.13	Reject	Reject
10	Helsinki	0	23	4.59	Reject	Reject
11	Hong Kong	0	33	5.57	Reject	Reject
12	Johannesburg	6	27	3.48	Reject	Reject
13	Lisbon	15	8	NA	NA	NA
14	London	7	26	3.13	Reject	Reject
15	Los Angeles	10	21	1.80	Reject	Do not reject
16	Luxembourg	0	23	4.59	Reject	Reject
17	Madrid	12	11	NA	NA	NA
18	Manila	7	26	3.13	Reject	Reject
19	Mexico City	2	31	4.87	Reject	Reject
20	Milan	0	23	4.59	Reject	Reject
21	Montreal	8	24	2.65	Reject	Do not reject
22	New York	2	29	4.67	Reject	Reject
23	Oslo	1	32	5.22	Reject	Reject
24	Paris	10	13	0.42	Do not reject	Do not reject
25	Rio de Janeiro	23	9	NA	NA	NA
26	Sao Paulo	21	11	NA	NA	NA
27	Singapore	1	32	5.22	Reject	Reject
28	Stockholm	0	33	5.57	Reject	Reject
29	Sydney	0	33	5.57	Reject	Reject
30	Tel Aviv	18	15	NA	NA	NA
31	Tokyo	2	31	4.87	Reject	Reject
32	Toronto	6	26	3.36	Reject	Reject
33	Vienna	2	21	3.75	Reject	Reject
34	Zurich	3	29	4.42	Reject	Reject

Table C.21: Dixon and Mood test based on Q^a_{adj}, for inter-country unskilled vs skilled

	City	# of pairs with $V(w_{ij})$ larger	# of pairs with $V(w_{ii^*})$ larger	Dixon & Mood test statistic	H0	H0 (Bonferroni)
1	Amsterdam	0	23	4.59	Reject	Reject
2	Bogota	6	27	3.48	Reject	Reject
3	Brussels	0	23	4.59	Reject	Reject
4	Buenos Aires	1	32	5.22	Reject	Reject
5	Caracas	26	7	NA	NA	NA
6	Chicago	5	26	3.59	Reject	Reject
7	Copenhagen	1	22	4.17	Reject	Reject
8	Dublin	0	23	4.59	Reject	Reject
9	Geneva	0	32	5.48	Reject	Reject
10	Helsinki	0	23	4.59	Reject	Reject
11	Hong Kong	0	33	5.57	Reject	Reject
12	Johannesburg	7	26	3.13	Reject	Reject
13	Lisbon	13	10	NA	NA	NA
14	London	2	31	4.87	Reject	Reject
15	Los Angeles	5	26	3.59	Reject	Reject
16	Luxembourg	0	23	4.59	Reject	Reject
17	Madrid	20	3	NA	NA	NA
18	Manila	9	24	2.44	Reject	Do not reject
19	Mexico City	0	33	5.57	Reject	Reject
20	Milan	0	23	4.59	Reject	Reject
21	Montreal	3	29	4.42	Reject	Reject
22	New York	1	30	5.03	Reject	Reject
23	Oslo	1	32	5.22	Reject	Reject
24	Paris	6	17	2.09	Reject	Do not reject
25	Rio de Janeiro	6	26	3.36	Reject	Reject
26	Sao Paulo	7	25	3.01	Reject	Reject
27	Singapore	0	33	5.57	Reject	Reject
28	Stockholm	0	33	5.57	Reject	Reject
29	Sydney	0	33	5.57	Reject	Reject
30	Tel Aviv	21	12	NA	NA	NA
31	Tokyo	1	32	5.22	Reject	Reject
32	Toronto	3	29	4.42	Reject	Reject
33	Vienna	0	23	4.59	Reject	Reject
34	Zurich	0	32	5.48	Reject	Reject

Table C.22: Summary of results of Dixon and Mood statistical sign test for each city with all of its valid inter-city pairs, excluding data from professions included in the survey after 1970.

Dixon & Mood test H0: $V(w_{ij}) > V(w_{ii^*})$	H0 rejected, $\alpha = 0.05$ (no multiple correction) % of cities			H0 rejected, $\alpha = 0.05/34$ (Bonferroni correction) % of cities		
Variability measures	ResVar	MAD	Q_adj	ResVar	MAD	Q_adj
All wage comparisons	$\frac{101}{136}$=96%	$\frac{129}{136}$=95%	$\frac{121}{136}$=89%	$\frac{126}{136}$=93%	$\frac{118}{136}$=87%	$\frac{115}{136}$=85%
Inter-country industry, vs services	$\frac{33}{34}$=97%	$\frac{34}{34}$=100%	$\frac{30}{34}$=88%	$\frac{33}{34}$=97%	$\frac{31}{34}$=94%	$\frac{29}{34}$=85%
Inter-country services, vs industry	$\frac{32}{34}$=94%	$\frac{31}{34}$=94%	$\frac{30}{34}$=88%	$\frac{30}{34}$=88%	$\frac{30}{34}$=88%	$\frac{28}{34}$=82%
Inter-country skilled vs unskilled	$\frac{32}{34}$=94%	$\frac{32}{34}$=94%	$\frac{30}{34}$=88%	$\frac{30}{34}$=88%	$\frac{26}{34}$=76%	$\frac{28}{34}$=82%
Inter-country unskilled vs skilled	$\frac{33}{34}$=97%	$\frac{32}{34}$=94%	$\frac{31}{34}$=91%	$\frac{33}{34}$=97%	$\frac{31}{34}$=91%	$\frac{30}{34}$=88%

Table C.23: Dixon and Mood test based on the variance of the regression residuals, for inter-country industry, vs services, excluding professions included in the survey only after 1970

	City	# of pairs with $V(w_{ij})$ larger	# of pairs with $V(w_{ii^*})$ larger	Dixon & Mood test statistic	H0	H0 (Bonferroni)
1	Amsterdam	0.00	23	4.59	Reject	Reject
2	Bogota	1.00	32	5.22	Reject	Reject
3	Brussels	0.00	23	4.59	Reject	Reject
4	Buenos Aires	0.00	33	5.57	Reject	Reject
5	Caracas	0.00	33	5.57	Reject	Reject
6	Chicago	0.00	31	5.39	Reject	Reject
7	Copenhagen	0.00	23	4.59	Reject	Reject
8	Dublin	0.00	23	4.59	Reject	Reject
9	Geneva	0.00	32	5.48	Reject	Reject
10	Helsinki	0.00	23	4.59	Reject	Reject
11	Hong Kong	1.00	32	5.22	Reject	Reject
12	Johannesburg	2.00	31	4.87	Reject	Reject
13	Lisbon	0.00	23	4.59	Reject	Reject
14	London	0.00	33	5.57	Reject	Reject
15	Los Angeles	0.00	31	5.39	Reject	Reject
16	Luxembourg	2.00	21	3.75	Reject	Reject
17	Madrid	0.00	23	4.59	Reject	Reject
18	Manila	0.00	33	5.57	Reject	Reject
19	Mexico City	0.00	33	5.57	Reject	Reject
20	Milan	0.00	23	4.59	Reject	Reject
21	Montreal	0.00	32	5.48	Reject	Reject
22	New York	0.00	31	5.39	Reject	Reject
23	Oslo	0.00	33	5.57	Reject	Reject
24	Paris	1.00	22	4.17	Reject	Reject
25	Rio de Janeiro	0.00	32	5.48	Reject	Reject
26	Sao Paulo	0.00	32	5.48	Reject	Reject
27	Singapore	0.00	33	5.57	Reject	Reject
28	Stockholm	0.00	33	5.57	Reject	Reject
29	Sydney	0.00	33	5.57	Reject	Reject
30	Tel Aviv	13.00	20	1.04	Do not reject	Do not reject
31	Tokyo	1.00	32	5.22	Reject	Reject
32	Toronto	2.00	30	4.77	Reject	Reject
33	Vienna	1.00	22	4.17	Reject	Reject
34	Zurich	5.00	27	3.71	Reject	Reject

Table C.24: Dixon and Mood test based on the variance of the regression residuals, for inter-country services, vs industry, excluding professions included in the survey only after 1970

	City	# of pairs with $V(w_{ij})$ larger	# of pairs with $V(w_{ii^*})$ larger	Dixon & Mood test statistic	H0	H0 (Bonferroni)
1	Amsterdam	0.00	23.00	4.59	Reject	Reject
2	Bogota	8.00	25.00	2.79	Reject	Do not reject
3	Brussels	0.00	23.00	4.59	Reject	Reject
4	Buenos Aires	0.00	33.00	5.57	Reject	Reject
5	Caracas	0.00	33.00	5.57	Reject	Reject
6	Chicago	1.00	30.00	5.03	Reject	Reject
7	Copenhagen	0.00	23.00	4.59	Reject	Reject
8	Dublin	3.00	20.00	3.34	Reject	Reject
9	Geneva	0.00	32.00	5.48	Reject	Reject
10	Helsinki	0.00	23.00	4.59	Reject	Reject
11	Hong Kong	33.00	0.00	NA	NA	NA
12	Johannesburg	8.00	25.00	2.79	Reject	Do not reject
13	Lisbon	1.00	22.00	4.17	Reject	Reject
14	London	0.00	33.00	5.57	Reject	Reject
15	Los Angeles	0.00	31.00	5.39	Reject	Reject
16	Luxembourg	2.00	21.00	3.75	Reject	Reject
17	Madrid	0.00	23.00	4.59	Reject	Reject
18	Manila	1.00	32.00	5.22	Reject	Reject
19	Mexico City	0.00	33.00	5.57	Reject	Reject
20	Milan	0.00	23.00	4.59	Reject	Reject
21	Montreal	0.00	32.00	5.48	Reject	Reject
22	New York	0.00	31.00	5.39	Reject	Reject
23	Oslo	1.00	32.00	5.22	Reject	Reject
24	Paris	2.00	21.00	3.75	Reject	Reject
25	Rio de Janeiro	0.00	32.00	5.48	Reject	Reject
26	Sao Paulo	0.00	32.00	5.48	Reject	Reject
27	Singapore	6.00	27.00	3.48	Reject	Reject
28	Stockholm	0.00	33.00	5.57	Reject	Reject
29	Sydney	0.00	33.00	5.57	Reject	Reject
30	Tel Aviv	16.00	17.00	0.00	Do not reject	Do not reject
31	Tokyo	0.00	33.00	5.57	Reject	Reject
32	Toronto	0.00	32.00	5.48	Reject	Reject
33	Vienna	0.00	23.00	4.59	Reject	Reject
34	Zurich	4.00	28.00	4.07	Reject	Reject

Table C.25: Dixon and Mood test based on the variance of the regression residuals, for inter-country skilled, vs unskilled, excluding professions included in the survey only after 1970

	City	# of pairs with $V(w_{ij})$ larger	# of pairs with $V(w_{ii^*})$ larger	Dixon & Mood test statistic	H0	H0 (Bonferroni)
1	Amsterdam	0.00	23	4.59	Reject	Reject
2	Bogota	1.00	32	5.22	Reject	Reject
3	Brussels	0.00	23	4.59	Reject	Reject
4	Buenos Aires	0.00	33	5.57	Reject	Reject
5	Caracas	0.00	33	5.57	Reject	Reject
6	Chicago	9.00	22	2.16	Reject	Do not reject
7	Copenhagen	0.00	23	4.59	Reject	Reject
8	Dublin	2.00	21	3.75	Reject	Reject
9	Geneva	1.00	31	5.13	Reject	Reject
10	Helsinki	0.00	23	4.59	Reject	Reject
11	Hong Kong	13.00	20	1.04	Do not reject	Do not reject
12	Johannesburg	5.00	28	3.83	Reject	Reject
13	Lisbon	1.00	22	4.17	Reject	Reject
14	London	0.00	33	5.57	Reject	Reject
15	Los Angeles	4.00	27	3.95	Reject	Reject
16	Luxembourg	0.00	23	4.59	Reject	Reject
17	Madrid	0.00	23	4.59	Reject	Reject
18	Manila	25.00	8	NA	NA	NA
19	Mexico City	0.00	33	5.57	Reject	Reject
20	Milan	0.00	23	4.59	Reject	Reject
21	Montreal	0.00	32	5.48	Reject	Reject
22	New York	0.00	31	5.39	Reject	Reject
23	Oslo	0.00	33	5.57	Reject	Reject
24	Paris	0.00	23	4.59	Reject	Reject
25	Rio de Janeiro	0.00	32	5.48	Reject	Reject
26	Sao Paulo	0.00	32	5.48	Reject	Reject
27	Singapore	0.00	33	5.57	Reject	Reject
28	Stockholm	0.00	33	5.57	Reject	Reject
29	Sydney	0.00	33	5.57	Reject	Reject
30	Tel Aviv	8.00	25	2.79	Reject	Do not reject
31	Tokyo	0.00	33	5.57	Reject	Reject
32	Toronto	0.00	32	5.48	Reject	Reject
33	Vienna	2.00	21	3.75	Reject	Reject
34	Zurich	0.00	32	5.48	Reject	Reject

Table C.26: Dixon and Mood test based on the variance of the regression residuals, for inter-country unskilled, vs skilled, excluding professions included in the survey only after 1970

	City	# of pairs with $V(w_{ij})$ larger	# of pairs with $V(w_{ii^*})$ larger	Dixon & Mood test statistic	H0	H0 (Bonferroni)
1	Amsterdam	3.00	20	3.34	Reject	Reject
2	Bogota	0.00	33	5.57	Reject	Reject
3	Brussels	1.00	22	4.17	Reject	Reject
4	Buenos Aires	2.00	31	4.87	Reject	Reject
5	Caracas	0.00	33	5.57	Reject	Reject
6	Chicago	2.00	29	4.67	Reject	Reject
7	Copenhagen	0.00	23	4.59	Reject	Reject
8	Dublin	0.00	23	4.59	Reject	Reject
9	Geneva	2.00	30	4.77	Reject	Reject
10	Helsinki	0.00	23	4.59	Reject	Reject
11	Hong Kong	0.00	33	5.57	Reject	Reject
12	Johannesburg	1.00	32	5.22	Reject	Reject
13	Lisbon	0.00	23	4.59	Reject	Reject
14	London	0.00	33	5.57	Reject	Reject
15	Los Angeles	1.00	30	5.03	Reject	Reject
16	Luxembourg	0.00	23	4.59	Reject	Reject
17	Madrid	3.00	20	3.34	Reject	Reject
18	Manila	12.00	21	1.39	Do not reject	Do not reject
19	Mexico City	0.00	33	5.57	Reject	Reject
20	Milan	0.00	23	4.59	Reject	Reject
21	Montreal	2.00	30	4.77	Reject	Reject
22	New York	2.00	29	4.67	Reject	Reject
23	Oslo	0.00	33	5.57	Reject	Reject
24	Paris	0.00	23	4.59	Reject	Reject
25	Rio de Janeiro	1.00	31	5.13	Reject	Reject
26	Sao Paulo	1.00	31	5.13	Reject	Reject
27	Singapore	0.00	33	5.57	Reject	Reject
28	Stockholm	0.00	33	5.57	Reject	Reject
29	Sydney	0.00	33	5.57	Reject	Reject
30	Tel Aviv	1.00	32	5.22	Reject	Reject
31	Tokyo	0.00	33	5.57	Reject	Reject
32	Toronto	1.00	31	5.13	Reject	Reject
33	Vienna	0.00	23	4.59	Reject	Reject
34	Zurich	0.00	32	5.48	Reject	Reject

Table C.27: Dixon and Mood test based on MAD, for inter-country industry, vs services, excluding professions included in the survey only after 1970

	City	# of pairs with $V(w_{ij})$ larger	# of pairs with $V(w_{ii^*})$ larger	Dixon & Mood test statistic	H0	H0 (Bonferroni)
1	Amsterdam	0.00	23	4.59	Reject	Reject
2	Bogota	1.00	32	5.22	Reject	Reject
3	Brussels	0.00	23	4.59	Reject	Reject
4	Buenos Aires	1.00	32	5.22	Reject	Reject
5	Caracas	0.00	33	5.57	Reject	Reject
6	Chicago	1.00	30	5.03	Reject	Reject
7	Copenhagen	0.00	23	4.59	Reject	Reject
8	Dublin	2.00	21	3.75	Reject	Reject
9	Geneva	1.00	31	5.13	Reject	Reject
10	Helsinki	0.00	23	4.59	Reject	Reject
11	Hong Kong	6.00	27	3.48	Reject	Reject
12	Johannesburg	0.00	33	5.57	Reject	Reject
13	Lisbon	0.00	23	4.59	Reject	Reject
14	London	4.00	29	4.18	Reject	Reject
15	Los Angeles	10.00	21	1.80	Reject	Do not reject
16	Luxembourg	3.00	20	3.34	Reject	Reject
17	Madrid	1.00	22	4.17	Reject	Reject
18	Manila	0.00	33	5.57	Reject	Reject
19	Mexico City	0.00	33	5.57	Reject	Reject
20	Milan	0.00	23	4.59	Reject	Reject
21	Montreal	3.00	29	4.42	Reject	Reject
22	New York	1.00	30	5.03	Reject	Reject
23	Oslo	0.00	33	5.57	Reject	Reject
24	Paris	4.00	19	2.92	Reject	Do not reject
25	Rio de Janeiro	2.00	30	4.77	Reject	Reject
26	Sao Paulo	0.00	32	5.48	Reject	Reject
27	Singapore	0.00	33	5.57	Reject	Reject
28	Stockholm	0.00	33	5.57	Reject	Reject
29	Sydney	2.00	31	4.87	Reject	Reject
30	Tel Aviv	2.00	31	4.87	Reject	Reject
31	Tokyo	1.00	32	5.22	Reject	Reject
32	Toronto	7.00	25	3.01	Reject	Reject
33	Vienna	2.00	21	3.75	Reject	Reject
34	Zurich	9.00	23	2.30	Reject	Do not reject

Table C.28: Dixon and Mood test based on MAD, for inter-country services, vs industry, excluding professions included in the survey only after 1970

	City	# of pairs with $V(w_{ij})$ larger	# of pairs with $V(w_{ii^*})$ larger	Dixon & Mood test statistic	H0	H0 (Bonferroni)
1	Amsterdam	0.00	23	4.59	Reject	Reject
2	Bogota	7.00	26	3.13	Reject	Reject
3	Brussels	1.00	22	4.17	Reject	Reject
4	Buenos Aires	0.00	33	5.57	Reject	Reject
5	Caracas	2.00	31	4.87	Reject	Reject
6	Chicago	4.00	27	3.95	Reject	Reject
7	Copenhagen	0.00	23	4.59	Reject	Reject
8	Dublin	3.00	20	3.34	Reject	Reject
9	Geneva	1.00	31	5.13	Reject	Reject
10	Helsinki	0.00	23	4.59	Reject	Reject
11	Hong Kong	22.00	11	NA	NA	NA
12	Johannesburg	0.00	33	5.57	Reject	Reject
13	Lisbon	2.00	21	3.75	Reject	Reject
14	London	2.00	31	4.87	Reject	Reject
15	Los Angeles	11.00	20	1.44	Do not reject	Do not reject
16	Luxembourg	1.00	22	4.17	Reject	Reject
17	Madrid	1.00	22	4.17	Reject	Reject
18	Manila	0.00	33	5.57	Reject	Reject
19	Mexico City	6.00	27	3.48	Reject	Reject
20	Milan	0.00	23	4.59	Reject	Reject
21	Montreal	5.00	27	3.71	Reject	Reject
22	New York	3.00	28	4.31	Reject	Reject
23	Oslo	3.00	30	4.53	Reject	Reject
24	Paris	4.00	19	2.92	Reject	Do not reject
25	Rio de Janeiro	2.00	30	4.77	Reject	Reject
26	Sao Paulo	0.00	32	5.48	Reject	Reject
27	Singapore	3.00	30	4.53	Reject	Reject
28	Stockholm	0.00	33	5.57	Reject	Reject
29	Sydney	0.00	33	5.57	Reject	Reject
30	Tel Aviv	1.00	32	5.22	Reject	Reject
31	Tokyo	1.00	32	5.22	Reject	Reject
32	Toronto	13.00	19	0.88	Do not reject	Do not reject
33	Vienna	1.00	22	4.17	Reject	Reject
34	Zurich	6.00	26	3.36	Reject	Reject

Table C.29: Dixon and Mood test based on MAD, for inter-country skilled, vs unskilled, excluding professions included in the survey only after 1970

	City	# of pairs with $V(w_{ij})$ larger	# of pairs with $V(w_{ii^*})$ larger	Dixon & Mood test statistic	H0	H0 (Bonferroni)
1	Amsterdam	0.00	23	4.59	Reject	Reject
2	Bogota	3.00	30	4.53	Reject	Reject
3	Brussels	0.00	23	4.59	Reject	Reject
4	Buenos Aires	0.00	33	5.57	Reject	Reject
5	Caracas	0.00	33	5.57	Reject	Reject
6	Chicago	1.00	30	5.03	Reject	Reject
7	Copenhagen	0.00	23	4.59	Reject	Reject
8	Dublin	1.00	22	4.17	Reject	Reject
9	Geneva	1.00	31	5.13	Reject	Reject
10	Helsinki	0.00	23	4.59	Reject	Reject
11	Hong Kong	9.00	24	2.44	Reject	Do not reject
12	Johannesburg	4.00	29	4.18	Reject	Reject
13	Lisbon	7.00	16	1.67	Reject	Do not reject
14	London	8.00	25	2.79	Reject	Do not reject
15	Los Angeles	3.00	28	4.31	Reject	Reject
16	Luxembourg	1.00	22	4.17	Reject	Reject
17	Madrid	4.00	19	2.92	Reject	Do not reject
18	Manila	11.00	22	1.74	Reject	Do not reject
19	Mexico City	3.00	30	4.53	Reject	Reject
20	Milan	0.00	23	4.59	Reject	Reject
21	Montreal	3.00	29	4.42	Reject	Reject
22	New York	0.00	31	5.39	Reject	Reject
23	Oslo	4.00	29	4.18	Reject	Reject
24	Paris	6.00	17	2.09	Reject	Do not reject
25	Rio de Janeiro	1.00	31	5.13	Reject	Reject
26	Sao Paulo	0.00	32	5.48	Reject	Reject
27	Singapore	0.00	33	5.57	Reject	Reject
28	Stockholm	0.00	33	5.57	Reject	Reject
29	Sydney	0.00	33	5.57	Reject	Reject
30	Tel Aviv	14.00	19	0.70	Do not reject	Do not reject
31	Tokyo	0.00	33	5.57	Reject	Reject
32	Toronto	10.00	22	1.94	Reject	Do not reject
33	Vienna	1.00	22	4.17	Reject	Reject
34	Zurich	0.00	32	5.48	Reject	Reject

Table C.30: Dixon and Mood test based on MAD, for inter-country unskilled, vs skilled, excluding professions included in the survey only after 1970

	City	# of pairs with $V(w_{ij})$ larger	# of pairs with $V(w_{ii^*})$ larger	Dixon & Mood test statistic	H0	H0 (Bonferroni)
1	Amsterdam	2.00	21	3.75	Reject	Reject
2	Bogota	0.00	33	5.57	Reject	Reject
3	Brussels	0.00	23	4.59	Reject	Reject
4	Buenos Aires	0.00	33	5.57	Reject	Reject
5	Caracas	0.00	33	5.57	Reject	Reject
6	Chicago	1.00	30	5.03	Reject	Reject
7	Copenhagen	0.00	23	4.59	Reject	Reject
8	Dublin	0.00	23	4.59	Reject	Reject
9	Geneva	1.00	31	5.13	Reject	Reject
10	Helsinki	0.00	23	4.59	Reject	Reject
11	Hong Kong	2.00	31	4.87	Reject	Reject
12	Johannesburg	0.00	33	5.57	Reject	Reject
13	Lisbon	3.00	20	3.34	Reject	Reject
14	London	6.00	27	3.48	Reject	Reject
15	Los Angeles	2.00	29	4.67	Reject	Reject
16	Luxembourg	2.00	21	3.75	Reject	Reject
17	Madrid	4.00	19	2.92	Reject	Do not reject
18	Manila	17.00	16	NA	NA	NA
19	Mexico City	1.00	32	5.22	Reject	Reject
20	Milan	0.00	23	4.59	Reject	Reject
21	Montreal	3.00	29	4.42	Reject	Reject
22	New York	2.00	29	4.67	Reject	Reject
23	Oslo	3.00	30	4.53	Reject	Reject
24	Paris	10.00	13	0.42	Do not reject	Do not reject
25	Rio de Janeiro	4.00	28	4.07	Reject	Reject
26	Sao Paulo	0.00	32	5.48	Reject	Reject
27	Singapore	0.00	33	5.57	Reject	Reject
28	Stockholm	0.00	33	5.57	Reject	Reject
29	Sydney	1.00	32	5.22	Reject	Reject
30	Tel Aviv	2.00	31	4.87	Reject	Reject
31	Tokyo	0.00	33	5.57	Reject	Reject
32	Toronto	6.00	26	3.36	Reject	Reject
33	Vienna	1.00	22	4.17	Reject	Reject
34	Zurich	1.00	31	5.13	Reject	Reject

Table C.31: Dixon and Mood test based on Q^a_{adj}, for inter-country industry, vs services, excluding professions included in the survey only after 1970

	City	# of pairs with $V(w_{ij})$ larger	# of pairs with $V(w_{ii^*})$ larger	Dixon & Mood test statistic	H0	H0 (Bonferroni)
1	Amsterdam	1.00	22	4.17	Reject	Reject
2	Bogota	1.00	32	5.22	Reject	Reject
3	Brussels	0.00	23	4.59	Reject	Reject
4	Buenos Aires	3.00	30	4.53	Reject	Reject
5	Caracas	1.00	32	5.22	Reject	Reject
6	Chicago	0.00	31	5.39	Reject	Reject
7	Copenhagen	0.00	23	4.59	Reject	Reject
8	Dublin	0.00	23	4.59	Reject	Reject
9	Geneva	0.00	32	5.48	Reject	Reject
10	Helsinki	0.00	23	4.59	Reject	Reject
11	Hong Kong	20.00	13	NA	NA	NA
12	Johannesburg	0.00	33	5.57	Reject	Reject
13	Lisbon	17.00	6	NA	NA	NA
14	London	2.00	31	4.87	Reject	Reject
15	Los Angeles	0.00	31	5.39	Reject	Reject
16	Luxembourg	3.00	20	3.34	Reject	Reject
17	Madrid	1.00	22	4.17	Reject	Reject
18	Manila	4.00	29	4.18	Reject	Reject
19	Mexico City	1.00	32	5.22	Reject	Reject
20	Milan	0.00	23	4.59	Reject	Reject
21	Montreal	0.00	32	5.48	Reject	Reject
22	New York	9.00	22	2.16	Reject	Do not reject
23	Oslo	2.00	31	4.87	Reject	Reject
24	Paris	3.00	20	3.34	Reject	Reject
25	Rio de Janeiro	0.00	32	5.48	Reject	Reject
26	Sao Paulo	0.00	32	5.48	Reject	Reject
27	Singapore	3.00	30	4.53	Reject	Reject
28	Stockholm	0.00	33	5.57	Reject	Reject
29	Sydney	7.00	26	3.13	Reject	Reject
30	Tel Aviv	22.00	11	NA	NA	NA
31	Tokyo	13.00	20	1.04	Do not reject	Do not reject
32	Toronto	3.00	29	4.42	Reject	Reject
33	Vienna	0.00	23	4.59	Reject	Reject
34	Zurich	0.00	32	5.48	Reject	Reject

Table C.32: Dixon and Mood test based on Q^a_{adj}, for inter-country services, vs industry, excluding professions included in the survey only after 1970

	City	# of pairs with $V(w_{ij})$ larger	# of pairs with $V(w_{ii^*})$ larger	Dixon & Mood test statistic	H0	H0 (Bonferroni)
1	Amsterdam	0.00	23	4.59	Reject	Reject
2	Bogota	2.00	31	4.87	Reject	Reject
3	Brussels	0.00	23	4.59	Reject	Reject
4	Buenos Aires	1.00	32	5.22	Reject	Reject
5	Caracas	3.00	30	4.53	Reject	Reject
6	Chicago	1.00	30	5.03	Reject	Reject
7	Copenhagen	0.00	23	4.59	Reject	Reject
8	Dublin	1.00	22	4.17	Reject	Reject
9	Geneva	0.00	32	5.48	Reject	Reject
10	Helsinki	0.00	23	4.59	Reject	Reject
11	Hong Kong	32.00	1	NA	NA	NA
12	Johannesburg	0.00	33	5.57	Reject	Reject
13	Lisbon	20.00	3	NA	NA	NA
14	London	0.00	33	5.57	Reject	Reject
15	Los Angeles	1.00	30	5.03	Reject	Reject
16	Luxembourg	3.00	20	3.34	Reject	Reject
17	Madrid	2.00	21	3.75	Reject	Reject
18	Manila	6.00	27	3.48	Reject	Reject
19	Mexico City	0.00	33	5.57	Reject	Reject
20	Milan	0.00	23	4.59	Reject	Reject
21	Montreal	0.00	32	5.48	Reject	Reject
22	New York	3.00	28	4.31	Reject	Reject
23	Oslo	10.00	23	2.09	Reject	Do not reject
24	Paris	0.00	23	4.59	Reject	Reject
25	Rio de Janeiro	0.00	32	5.48	Reject	Reject
26	Sao Paulo	0.00	32	5.48	Reject	Reject
27	Singapore	9.00	24	2.44	Reject	Do not reject
28	Stockholm	1.00	32	5.22	Reject	Reject
29	Sydney	4.00	29	4.18	Reject	Reject
30	Tel Aviv	23.00	10	NA	NA	NA
31	Tokyo	12.00	21	1.39	Do not reject	Do not reject
32	Toronto	2.00	30	4.77	Reject	Reject
33	Vienna	0.00	23	4.59	Reject	Reject
34	Zurich	5.00	27	3.71	Reject	Reject

Table C.33: Dixon and Mood test based on Q^a_{adj}, for inter-country skilled vs unskilled, excluding professions included in the survey only after 1970

	City	# of pairs with $V(w_{ij})$ larger	# of pairs with $V(w_{ii^*})$ larger	Dixon & Mood test statistic	H0	H0 (Bonferroni)
1	Amsterdam	2.00	21	3.75	Reject	Reject
2	Bogota	1.00	32	5.22	Reject	Reject
3	Brussels	0.00	23	4.59	Reject	Reject
4	Buenos Aires	2.00	31	4.87	Reject	Reject
5	Caracas	31.00	2	NA	NA	NA
6	Chicago	15.00	16	0.00	Do not reject	Do not reject
7	Copenhagen	2.00	21	3.75	Reject	Reject
8	Dublin	0.00	23	4.59	Reject	Reject
9	Geneva	1.00	31	5.13	Reject	Reject
10	Helsinki	1.00	22	4.17	Reject	Reject
11	Hong Kong	0.00	33	5.57	Reject	Reject
12	Johannesburg	1.00	32	5.22	Reject	Reject
13	Lisbon	18.00	5	NA	NA	NA
14	London	2.00	31	4.87	Reject	Reject
15	Los Angeles	4.00	27	3.95	Reject	Reject
16	Luxembourg	1.00	22	4.17	Reject	Reject
17	Madrid	10.00	13	0.42	Do not reject	Do not reject
18	Manila	10.00	23	2.09	Reject	Do not reject
19	Mexico City	0.00	33	5.57	Reject	Reject
20	Milan	0.00	23	4.59	Reject	Reject
21	Montreal	2.00	30	4.77	Reject	Reject
22	New York	0.00	31	5.39	Reject	Reject
23	Oslo	1.00	32	5.22	Reject	Reject
24	Paris	1.00	22	4.17	Reject	Reject
25	Rio de Janeiro	8.00	24	2.65	Reject	Do not reject
26	Sao Paulo	1.00	31	5.13	Reject	Reject
27	Singapore	0.00	33	5.57	Reject	Reject
28	Stockholm	0.00	33	5.57	Reject	Reject
29	Sydney	0.00	33	5.57	Reject	Reject
30	Tel Aviv	2.00	31	4.87	Reject	Reject
31	Tokyo	4.00	29	4.18	Reject	Reject
32	Toronto	3.00	29	4.42	Reject	Reject
33	Vienna	0.00	23	4.59	Reject	Reject
34	Zurich	4.00	28	4.07	Reject	Reject

Table C.34: Dixon and Mood test based on Q^a_{adj}, for inter-country unskilled vs skilled, excluding professions included in the survey only after 1970

	City	# of pairs with $V(w_{ij})$ larger	# of pairs with $V(w_{ii^*})$ larger	Dixon & Mood test statistic	H0	H0 (Bonferroni)
1	Amsterdam	3.00	20	3.34	Reject	Reject
2	Bogota	0.00	33	5.57	Reject	Reject
3	Brussels	0.00	23	4.59	Reject	Reject
4	Buenos Aires	4.00	29	4.18	Reject	Reject
5	Caracas	19.00	14	NA	NA	NA
6	Chicago	5.00	26	3.59	Reject	Reject
7	Copenhagen	1.00	22	4.17	Reject	Reject
8	Dublin	0.00	23	4.59	Reject	Reject
9	Geneva	1.00	31	5.13	Reject	Reject
10	Helsinki	0.00	23	4.59	Reject	Reject
11	Hong Kong	1.00	32	5.22	Reject	Reject
12	Johannesburg	2.00	31	4.87	Reject	Reject
13	Lisbon	17.00	6	NA	NA	NA
14	London	1.00	32	5.22	Reject	Reject
15	Los Angeles	6.00	25	3.23	Reject	Reject
16	Luxembourg	1.00	22	4.17	Reject	Reject
17	Madrid	10.00	13	0.42	Do not reject	Do not reject
18	Manila	9.00	24	2.44	Reject	Do not reject
19	Mexico City	0.00	33	5.57	Reject	Reject
20	Milan	0.00	23	4.59	Reject	Reject
21	Montreal	4.00	28	4.07	Reject	Reject
22	New York	5.00	26	3.59	Reject	Reject
23	Oslo	1.00	32	5.22	Reject	Reject
24	Paris	2.00	21	3.75	Reject	Reject
25	Rio de Janeiro	7.00	25	3.01	Reject	Reject
26	Sao Paulo	0.00	32	5.48	Reject	Reject
27	Singapore	0.00	33	5.57	Reject	Reject
28	Stockholm	2.00	31	4.87	Reject	Reject
29	Sydney	0.00	33	5.57	Reject	Reject
30	Tel Aviv	0.00	33	5.57	Reject	Reject
31	Tokyo	6.00	27	3.48	Reject	Reject
32	Toronto	5.00	27	3.71	Reject	Reject
33	Vienna	0.00	23	4.59	Reject	Reject
34	Zurich	2.00	30	4.77	Reject	Reject

Table C.35: Summary of results of Dixon and Mood statistical sign test for each city with all of its valid inter-city pairs, 1976-2009 data while holding the composition of the indices constant.

Dixon & Mood test H0: $V(w_{ij}) > V(w_{ii^*})$	H0 rejected, $\alpha = 0.05$ (no multiple correction) % of cities			H0 rejected, $\alpha = 0.05/34$ (Bonferroni correction) % of cities		
Variability measures	ResVar	MAD	Q_adj	ResVar	MAD	Q_adj
All wage comparisons	$\frac{198}{204}$=97%	$\frac{192}{204}$=94%	$\frac{174}{204}$=85%	$\frac{192}{204}$=94%	$\frac{182}{204}$=89%	$\frac{161}{204}$=79%
Inter-country industry, vs services	$\frac{34}{34}$=100%	$\frac{32}{34}$=94%	$\frac{29}{34}$=85%	$\frac{33}{34}$=97%	$\frac{31}{34}$=91%	$\frac{27}{34}$=79%
Inter-country services, vs industry	$\frac{32}{34}$=94%	$\frac{33}{34}$=97%	$\frac{29}{34}$=85%	$\frac{31}{34}$=91%	$\frac{29}{34}$=85%	$\frac{27}{34}$=79%
Inter-country competitive vs uncompetitive	$\frac{34}{34}$=100%	$\frac{32}{34}$=94%	$\frac{31}{34}$=91%	$\frac{34}{34}$=100%	$\frac{30}{34}$=88%	$\frac{27}{34}$=79%
Inter-country uncompetitive vs competitive	$\frac{31}{34}$=91%	$\frac{30}{34}$=88%	$\frac{26}{34}$=76%	$\frac{28}{34}$=82%	$\frac{27}{34}$=79%	$\frac{24}{34}$=71%
Inter-country skilled vs unskilled	$\frac{34}{34}$=100%	$\frac{32}{34}$=94%	$\frac{30}{34}$=88%	$\frac{34}{34}$=100%	$\frac{32}{34}$=94%	$\frac{28}{34}$=82%
Inter-country unskilled vs skilled	$\frac{33}{34}$=97%	$\frac{33}{34}$=97%	$\frac{29}{34}$=%	$\frac{32}{34}$=94%	$\frac{33}{34}$=97%	$\frac{28}{34}$=82%

Table C.36: Dixon and Mood test based on the variance of the regression residuals, for inter-country industry, vs services, 1976-2009 data while holding the composition of the indices constant

	City	# of pairs with $V(w_{ij})$ larger	# of pairs with $V(w_{ii^*})$ larger	Dixon & Mood test statistic	H0	H0 (Bonferroni)
1	Amsterdam	0.00	23	4.59	Reject	Reject
2	Bogota	7.00	26	3.13	Reject	Reject
3	Brussels	0.00	23	4.59	Reject	Reject
4	Buenos Aires	0.00	33	5.57	Reject	Reject
5	Caracas	0.00	33	5.57	Reject	Reject
6	Chicago	1.00	30	5.03	Reject	Reject
7	Copenhagen	0.00	23	4.59	Reject	Reject
8	Dublin	0.00	23	4.59	Reject	Reject
9	Geneva	1.00	31	5.13	Reject	Reject
10	Helsinki	0.00	23	4.59	Reject	Reject
11	Hong Kong	1.00	32	5.22	Reject	Reject
12	Johannesburg	0.00	33	5.57	Reject	Reject
13	Lisbon	0.00	23	4.59	Reject	Reject
14	London	0.00	33	5.57	Reject	Reject
15	Los Angeles	0.00	31	5.39	Reject	Reject
16	Luxembourg	0.00	23	4.59	Reject	Reject
17	Madrid	0.00	23	4.59	Reject	Reject
18	Manila	1.00	32	5.22	Reject	Reject
19	Mexico City	0.00	33	5.57	Reject	Reject
20	Milan	0.00	23	4.59	Reject	Reject
21	Montreal	0.00	32	5.48	Reject	Reject
22	New York	2.00	29	4.67	Reject	Reject
23	Oslo	0.00	33	5.57	Reject	Reject
24	Paris	5.00	18	2.50	Reject	Do not reject
25	Rio de Janeiro	0.00	32	5.48	Reject	Reject
26	Sao Paulo	0.00	32	5.48	Reject	Reject
27	Singapore	1.00	32	5.22	Reject	Reject
28	Stockholm	0.00	33	5.57	Reject	Reject
29	Sydney	0.00	33	5.57	Reject	Reject
30	Tel Aviv	1.00	32	5.22	Reject	Reject
31	Tokyo	0.00	33	5.57	Reject	Reject
32	Toronto	1.00	31	5.13	Reject	Reject
33	Vienna	1.00	22	4.17	Reject	Reject
34	Zurich	1.00	31	5.13	Reject	Reject

Table C.37: Dixon and Mood test based on the variance of the regression residuals, for inter-country services, vs industry, 1976-2009 data while holding the composition of the indices constant

	City	# of pairs with $V(w_{ij})$ larger	# of pairs with $V(w_{ii^*})$ larger	Dixon & Mood test statistic	H0	H0 (Bonferroni)
1	Amsterdam	0.00	23	4.59	Reject	Reject
2	Bogota	3.00	30	4.53	Reject	Reject
3	Brussels	1.00	22	4.17	Reject	Reject
4	Buenos Aires	0.00	33	5.57	Reject	Reject
5	Caracas	0.00	33	5.57	Reject	Reject
6	Chicago	6.00	25	3.23	Reject	Reject
7	Copenhagen	0.00	23	4.59	Reject	Reject
8	Dublin	2.00	21	3.75	Reject	Reject
9	Geneva	1.00	31	5.13	Reject	Reject
10	Helsinki	0.00	23	4.59	Reject	Reject
11	Hong Kong	17.00	16	NA	NA	NA
12	Johannesburg	0.00	33	5.57	Reject	Reject
13	Lisbon	0.00	23	4.59	Reject	Reject
14	London	0.00	33	5.57	Reject	Reject
15	Los Angeles	0.00	31	5.39	Reject	Reject
16	Luxembourg	0.00	23	4.59	Reject	Reject
17	Madrid	0.00	23	4.59	Reject	Reject
18	Manila	21.00	12	NA	NA	NA
19	Mexico City	0.00	33	5.57	Reject	Reject
20	Milan	0.00	23	4.59	Reject	Reject
21	Montreal	0.00	32	5.48	Reject	Reject
22	New York	1.00	30	5.03	Reject	Reject
23	Oslo	0.00	33	5.57	Reject	Reject
24	Paris	6.00	17	2.09	Reject	Do not reject
25	Rio de Janeiro	0.00	32	5.48	Reject	Reject
26	Sao Paulo	1.00	31	5.13	Reject	Reject
27	Singapore	5.00	28	3.83	Reject	Reject
28	Stockholm	0.00	33	5.57	Reject	Reject
29	Sydney	1.00	32	5.22	Reject	Reject
30	Tel Aviv	0.00	33	5.57	Reject	Reject
31	Tokyo	0.00	33	5.57	Reject	Reject
32	Toronto	0.00	32	5.48	Reject	Reject
33	Vienna	2.00	21	3.75	Reject	Reject
34	Zurich	0.00	32	5.48	Reject	Reject

Table C.38: Dixon and Mood test based on the variance of the regression residuals, for inter-country competitive, vs uncompetitive, 1976-2009 data while holding the composition of the indices constant

	City	# of pairs with $V(w_{ij})$ larger	# of pairs with $V(w_{ii^*})$ larger	Dixon & Mood test statistic	H0	H0 (Bonferroni)
1	Amsterdam	0.00	23	4.59	Reject	Reject
2	Bogota	1.00	32	5.22	Reject	Reject
3	Brussels	0.00	23	4.59	Reject	Reject
4	Buenos Aires	0.00	33	5.57	Reject	Reject
5	Caracas	0.00	33	5.57	Reject	Reject
6	Chicago	1.00	30	5.03	Reject	Reject
7	Copenhagen	0.00	23	4.59	Reject	Reject
8	Dublin	0.00	23	4.59	Reject	Reject
9	Geneva	1.00	31	5.13	Reject	Reject
10	Helsinki	0.00	23	4.59	Reject	Reject
11	Hong Kong	1.00	32	5.22	Reject	Reject
12	Johannesburg	0.00	33	5.57	Reject	Reject
13	Lisbon	0.00	23	4.59	Reject	Reject
14	London	0.00	33	5.57	Reject	Reject
15	Los Angeles	0.00	31	5.39	Reject	Reject
16	Luxembourg	0.00	23	4.59	Reject	Reject
17	Madrid	0.00	23	4.59	Reject	Reject
18	Manila	0.00	33	5.57	Reject	Reject
19	Mexico City	0.00	33	5.57	Reject	Reject
20	Milan	0.00	23	4.59	Reject	Reject
21	Montreal	0.00	32	5.48	Reject	Reject
22	New York	1.00	30	5.03	Reject	Reject
23	Oslo	5.00	28	3.83	Reject	Reject
24	Paris	3.00	20	3.34	Reject	Reject
25	Rio de Janeiro	0.00	32	5.48	Reject	Reject
26	Sao Paulo	0.00	32	5.48	Reject	Reject
27	Singapore	1.00	32	5.22	Reject	Reject
28	Stockholm	0.00	33	5.57	Reject	Reject
29	Sydney	0.00	33	5.57	Reject	Reject
30	Tel Aviv	1.00	32	5.22	Reject	Reject
31	Tokyo	0.00	33	5.57	Reject	Reject
32	Toronto	0.00	32	5.48	Reject	Reject
33	Vienna	0.00	23	4.59	Reject	Reject
34	Zurich	0.00	32	5.48	Reject	Reject

Table C.39: Dixon and Mood test based on the variance of the regression residuals, for inter-country uncompetitive, vs competitive, 1976-2009 data while holding the composition of the indices constant

	City	# of pairs with $V(w_{ij})$ larger	# of pairs with $V(w_{ii^*})$ larger	Dixon & Mood test statistic	H0	H0 (Bonferroni)
1	Amsterdam	0.00	23	4.59	Reject	Reject
2	Bogota	4.00	29	4.18	Reject	Reject
3	Brussels	4.00	19	2.92	Reject	Do not reject
4	Buenos Aires	7.00	26	3.13	Reject	Reject
5	Caracas	0.00	33	5.57	Reject	Reject
6	Chicago	20.00	11	NA	NA	NA
7	Copenhagen	0.00	23	4.59	Reject	Reject
8	Dublin	3.00	20	3.34	Reject	Reject
9	Geneva	11.00	21	1.59	Do not reject	Do not reject
10	Helsinki	0.00	23	4.59	Reject	Reject
11	Hong Kong	11.00	22	1.74	Reject	Do not reject
12	Johannesburg	1.00	32	5.22	Reject	Reject
13	Lisbon	2.00	21	3.75	Reject	Reject
14	London	0.00	33	5.57	Reject	Reject
15	Los Angeles	1.00	30	5.03	Reject	Reject
16	Luxembourg	2.00	21	3.75	Reject	Reject
17	Madrid	3.00	20	3.34	Reject	Reject
18	Manila	25.00	8	NA	NA	NA
19	Mexico City	0.00	33	5.57	Reject	Reject
20	Milan	2.00	21	3.75	Reject	Reject
21	Montreal	0.00	32	5.48	Reject	Reject
22	New York	2.00	29	4.67	Reject	Reject
23	Oslo	7.00	26	3.13	Reject	Reject
24	Paris	6.00	17	2.09	Reject	Do not reject
25	Rio de Janeiro	0.00	32	5.48	Reject	Reject
26	Sao Paulo	3.00	29	4.42	Reject	Reject
27	Singapore	5.00	28	3.83	Reject	Reject
28	Stockholm	1.00	32	5.22	Reject	Reject
29	Sydney	1.00	32	5.22	Reject	Reject
30	Tel Aviv	0.00	33	5.57	Reject	Reject
31	Tokyo	0.00	33	5.57	Reject	Reject
32	Toronto	0.00	32	5.48	Reject	Reject
33	Vienna	2.00	21	3.75	Reject	Reject
34	Zurich	0.00	32	5.48	Reject	Reject

Table C.40: Dixon and Mood test based on the variance of the regression residuals, for inter-country skilled, vs unskilled, 1976-2009 data while holding the composition of the indices constant

	City	# of pairs with $V(w_{ij})$ larger	# of pairs with $V(w_{ii^*})$ larger	Dixon & Mood test statistic	H0	H0 (Bonferroni)
1	Amsterdam	0.00	23	4.59	Reject	Reject
2	Bogota	1.00	32	5.22	Reject	Reject
3	Brussels	0.00	23	4.59	Reject	Reject
4	Buenos Aires	0.00	33	5.57	Reject	Reject
5	Caracas	0.00	33	5.57	Reject	Reject
6	Chicago	0.00	31	5.39	Reject	Reject
7	Copenhagen	0.00	23	4.59	Reject	Reject
8	Dublin	1.00	22	4.17	Reject	Reject
9	Geneva	2.00	30	4.77	Reject	Reject
10	Helsinki	0.00	23	4.59	Reject	Reject
11	Hong Kong	3.00	30	4.53	Reject	Reject
12	Johannesburg	7.00	26	3.13	Reject	Reject
13	Lisbon	1.00	22	4.17	Reject	Reject
14	London	0.00	33	5.57	Reject	Reject
15	Los Angeles	2.00	29	4.67	Reject	Reject
16	Luxembourg	2.00	21	3.75	Reject	Reject
17	Madrid	0.00	23	4.59	Reject	Reject
18	Manila	0.00	33	5.57	Reject	Reject
19	Mexico City	0.00	33	5.57	Reject	Reject
20	Milan	0.00	23	4.59	Reject	Reject
21	Montreal	1.00	31	5.13	Reject	Reject
22	New York	0.00	31	5.39	Reject	Reject
23	Oslo	0.00	33	5.57	Reject	Reject
24	Paris	0.00	23	4.59	Reject	Reject
25	Rio de Janeiro	0.00	32	5.48	Reject	Reject
26	Sao Paulo	2.00	30	4.77	Reject	Reject
27	Singapore	5.00	28	3.83	Reject	Reject
28	Stockholm	0.00	33	5.57	Reject	Reject
29	Sydney	0.00	33	5.57	Reject	Reject
30	Tel Aviv	5.00	28	3.83	Reject	Reject
31	Tokyo	0.00	33	5.57	Reject	Reject
32	Toronto	0.00	32	5.48	Reject	Reject
33	Vienna	2.00	21	3.75	Reject	Reject
34	Zurich	0.00	32	5.48	Reject	Reject

Table C.41: Dixon and Mood test based on the variance of the regression residuals, for inter-country unskilled, vs skilled, 1976-2009 data while holding the composition of the indices constant

	City	# of pairs with $V(w_{ij})$ larger	# of pairs with $V(w_{ii^*})$ larger	Dixon & Mood test statistic	H0	H0 (Bonferroni)
1	Amsterdam	0.00	23	4.59	Reject	Reject
2	Bogota	1.00	32	5.22	Reject	Reject
3	Brussels	0.00	23	4.59	Reject	Reject
4	Buenos Aires	4.00	29	4.18	Reject	Reject
5	Caracas	0.00	33	5.57	Reject	Reject
6	Chicago	0.00	31	5.39	Reject	Reject
7	Copenhagen	0.00	23	4.59	Reject	Reject
8	Dublin	1.00	22	4.17	Reject	Reject
9	Geneva	0.00	32	5.48	Reject	Reject
10	Helsinki	0.00	23	4.59	Reject	Reject
11	Hong Kong	0.00	33	5.57	Reject	Reject
12	Johannesburg	5.00	28	3.83	Reject	Reject
13	Lisbon	0.00	23	4.59	Reject	Reject
14	London	0.00	33	5.57	Reject	Reject
15	Los Angeles	2.00	29	4.67	Reject	Reject
16	Luxembourg	0.00	23	4.59	Reject	Reject
17	Madrid	2.00	21	3.75	Reject	Reject
18	Manila	8.00	25	2.79	Reject	Do not reject
19	Mexico City	0.00	33	5.57	Reject	Reject
20	Milan	0.00	23	4.59	Reject	Reject
21	Montreal	2.00	30	4.77	Reject	Reject
22	New York	1.00	30	5.03	Reject	Reject
23	Oslo	0.00	33	5.57	Reject	Reject
24	Paris	1.00	22	4.17	Reject	Reject
25	Rio de Janeiro	0.00	32	5.48	Reject	Reject
26	Sao Paulo	18.00	14	NA	NA	NA
27	Singapore	0.00	33	5.57	Reject	Reject
28	Stockholm	0.00	33	5.57	Reject	Reject
29	Sydney	0.00	33	5.57	Reject	Reject
30	Tel Aviv	2.00	31	4.87	Reject	Reject
31	Tokyo	0.00	33	5.57	Reject	Reject
32	Toronto	1.00	31	5.13	Reject	Reject
33	Vienna	0.00	23	4.59	Reject	Reject
34	Zurich	0.00	32	5.48	Reject	Reject

Table C.42: Dixon and Mood test based on MAD, for inter-country industry, vs services, 1976-2009 data while holding the composition of the indices constant

	City	# of pairs with $V(w_{ij})$ larger	# of pairs with $V(w_{ii^*})$ larger	Dixon & Mood test statistic	H0	H0 (Bonferroni)
1	Amsterdam	1.00	22	4.17	Reject	Reject
2	Bogota	4.00	29	4.18	Reject	Reject
3	Brussels	3.00	20	3.34	Reject	Reject
4	Buenos Aires	1.00	32	5.22	Reject	Reject
5	Caracas	0.00	33	5.57	Reject	Reject
6	Chicago	3.00	28	4.31	Reject	Reject
7	Copenhagen	0.00	23	4.59	Reject	Reject
8	Dublin	0.00	23	4.59	Reject	Reject
9	Geneva	1.00	31	5.13	Reject	Reject
10	Helsinki	1.00	22	4.17	Reject	Reject
11	Hong Kong	3.00	30	4.53	Reject	Reject
12	Johannesburg	13.00	20	1.04	Do not reject	Do not reject
13	Lisbon	0.00	23	4.59	Reject	Reject
14	London	0.00	33	5.57	Reject	Reject
15	Los Angeles	2.00	29	4.67	Reject	Reject
16	Luxembourg	1.00	22	4.17	Reject	Reject
17	Madrid	5.00	18	2.50	Reject	Do not reject
18	Manila	5.00	28	3.83	Reject	Reject
19	Mexico City	0.00	33	5.57	Reject	Reject
20	Milan	0.00	23	4.59	Reject	Reject
21	Montreal	5.00	27	3.71	Reject	Reject
22	New York	1.00	30	5.03	Reject	Reject
23	Oslo	2.00	31	4.87	Reject	Reject
24	Paris	10.00	13	0.42	Do not reject	Do not reject
25	Rio de Janeiro	1.00	31	5.13	Reject	Reject
26	Sao Paulo	0.00	32	5.48	Reject	Reject
27	Singapore	1.00	32	5.22	Reject	Reject
28	Stockholm	0.00	33	5.57	Reject	Reject
29	Sydney	0.00	33	5.57	Reject	Reject
30	Tel Aviv	6.00	27	3.48	Reject	Reject
31	Tokyo	0.00	33	5.57	Reject	Reject
32	Toronto	2.00	30	4.77	Reject	Reject
33	Vienna	1.00	22	4.17	Reject	Reject
34	Zurich	1.00	31	5.13	Reject	Reject

Table C.43: Dixon and Mood test based on MAD, for inter-country services, vs industry, 1976-2009 data while holding the composition of the indices constant

	City	# of pairs with $V(w_{ij})$ larger	# of pairs with $V(w_{ii^*})$ larger	Dixon & Mood test statistic	H0	H0 (Bonferroni)
1	Amsterdam	0.00	23	4.59	Reject	Reject
2	Bogota	7.00	26	3.13	Reject	Reject
3	Brussels	5.00	18	2.50	Reject	Do not reject
4	Buenos Aires	0.00	33	5.57	Reject	Reject
5	Caracas	0.00	33	5.57	Reject	Reject
6	Chicago	11.00	20	1.44	Do not reject	Do not reject
7	Copenhagen	0.00	23	4.59	Reject	Reject
8	Dublin	0.00	23	4.59	Reject	Reject
9	Geneva	1.00	31	5.13	Reject	Reject
10	Helsinki	0.00	23	4.59	Reject	Reject
11	Hong Kong	5.00	28	3.83	Reject	Reject
12	Johannesburg	3.00	30	4.53	Reject	Reject
13	Lisbon	0.00	23	4.59	Reject	Reject
14	London	0.00	33	5.57	Reject	Reject
15	Los Angeles	1.00	30	5.03	Reject	Reject
16	Luxembourg	0.00	23	4.59	Reject	Reject
17	Madrid	3.00	20	3.34	Reject	Reject
18	Manila	8.00	25	2.79	Reject	Do not reject
19	Mexico City	3.00	30	4.53	Reject	Reject
20	Milan	0.00	23	4.59	Reject	Reject
21	Montreal	8.00	24	2.65	Reject	Do not reject
22	New York	2.00	29	4.67	Reject	Reject
23	Oslo	0.00	33	5.57	Reject	Reject
24	Paris	7.00	16	1.67	Reject	Do not reject
25	Rio de Janeiro	0.00	32	5.48	Reject	Reject
26	Sao Paulo	0.00	32	5.48	Reject	Reject
27	Singapore	3.00	30	4.53	Reject	Reject
28	Stockholm	0.00	33	5.57	Reject	Reject
29	Sydney	1.00	32	5.22	Reject	Reject
30	Tel Aviv	2.00	31	4.87	Reject	Reject
31	Tokyo	1.00	32	5.22	Reject	Reject
32	Toronto	5.00	27	3.71	Reject	Reject
33	Vienna	0.00	23	4.59	Reject	Reject
34	Zurich	0.00	32	5.48	Reject	Reject

Table C.44: Dixon and Mood test based on MAD, for inter-country competitive, vs uncompetitive, 1976-2009 data while holding the composition of the indices constant

	City	# of pairs with $V(w_{ij})$ larger	# of pairs with $V(w_{ii^*})$ larger	Dixon & Mood test statistic	H0	H0 (Bonferroni)
1	Amsterdam	1.00	22	4.17	Reject	Reject
2	Bogota	1.00	32	5.22	Reject	Reject
3	Brussels	5.00	18	2.50	Reject	Do not reject
4	Buenos Aires	0.00	33	5.57	Reject	Reject
5	Caracas	0.00	33	5.57	Reject	Reject
6	Chicago	2.00	29	4.67	Reject	Reject
7	Copenhagen	0.00	23	4.59	Reject	Reject
8	Dublin	0.00	23	4.59	Reject	Reject
9	Geneva	0.00	32	5.48	Reject	Reject
10	Helsinki	2.00	21	3.75	Reject	Reject
11	Hong Kong	9.00	24	2.44	Reject	Do not reject
12	Johannesburg	19.00	14	NA	NA	NA
13	Lisbon	2.00	21	3.75	Reject	Reject
14	London	0.00	33	5.57	Reject	Reject
15	Los Angeles	0.00	31	5.39	Reject	Reject
16	Luxembourg	0.00	23	4.59	Reject	Reject
17	Madrid	2.00	21	3.75	Reject	Reject
18	Manila	4.00	29	4.18	Reject	Reject
19	Mexico City	0.00	33	5.57	Reject	Reject
20	Milan	0.00	23	4.59	Reject	Reject
21	Montreal	3.00	29	4.42	Reject	Reject
22	New York	1.00	30	5.03	Reject	Reject
23	Oslo	6.00	27	3.48	Reject	Reject
24	Paris	10.00	13	0.42	Do not reject	Do not reject
25	Rio de Janeiro	1.00	31	5.13	Reject	Reject
26	Sao Paulo	0.00	32	5.48	Reject	Reject
27	Singapore	0.00	33	5.57	Reject	Reject
28	Stockholm	2.00	31	4.87	Reject	Reject
29	Sydney	1.00	32	5.22	Reject	Reject
30	Tel Aviv	1.00	32	5.22	Reject	Reject
31	Tokyo	0.00	33	5.57	Reject	Reject
32	Toronto	0.00	32	5.48	Reject	Reject
33	Vienna	1.00	22	4.17	Reject	Reject
34	Zurich	0.00	32	5.48	Reject	Reject

Table C.45: Dixon and Mood test based on MAD, for inter-country uncompetitive, vs competitive, 1976-2009 data while holding the composition of the indices constant

	City	# of pairs with $V(w_{ij})$ larger	# of pairs with $V(w_{ii^*})$ larger	Dixon & Mood test statistic	H0	H0 (Bonferroni)
1	Amsterdam	1.00	22	4.17	Reject	Reject
2	Bogota	3.00	30	4.53	Reject	Reject
3	Brussels	9.00	14	0.83	Do not reject	Do not reject
4	Buenos Aires	0.00	33	5.57	Reject	Reject
5	Caracas	0.00	33	5.57	Reject	Reject
6	Chicago	16.00	15	NA	NA	NA
7	Copenhagen	0.00	23	4.59	Reject	Reject
8	Dublin	1.00	22	4.17	Reject	Reject
9	Geneva	2.00	30	4.77	Reject	Reject
10	Helsinki	0.00	23	4.59	Reject	Reject
11	Hong Kong	9.00	24	2.44	Reject	Do not reject
12	Johannesburg	8.00	25	2.79	Reject	Do not reject
13	Lisbon	1.00	22	4.17	Reject	Reject
14	London	0.00	33	5.57	Reject	Reject
15	Los Angeles	2.00	29	4.67	Reject	Reject
16	Luxembourg	1.00	22	4.17	Reject	Reject
17	Madrid	2.00	21	3.75	Reject	Reject
18	Manila	13.00	20	1.04	Do not reject	Do not reject
19	Mexico City	0.00	33	5.57	Reject	Reject
20	Milan	1.00	22	4.17	Reject	Reject
21	Montreal	4.00	28	4.07	Reject	Reject
22	New York	0.00	31	5.39	Reject	Reject
23	Oslo	10.00	23	2.09	Reject	Do not reject
24	Paris	8.00	15	1.25	Do not reject	Do not reject
25	Rio de Janeiro	0.00	32	5.48	Reject	Reject
26	Sao Paulo	0.00	32	5.48	Reject	Reject
27	Singapore	2.00	31	4.87	Reject	Reject
28	Stockholm	4.00	29	4.18	Reject	Reject
29	Sydney	7.00	26	3.13	Reject	Reject
30	Tel Aviv	0.00	33	5.57	Reject	Reject
31	Tokyo	0.00	33	5.57	Reject	Reject
32	Toronto	2.00	30	4.77	Reject	Reject
33	Vienna	3.00	20	3.34	Reject	Reject
34	Zurich	0.00	32	5.48	Reject	Reject

Table C.46: Dixon and Mood test based on MAD, for inter-country skilled, vs unskilled, 1976-2009 data while holding the composition of the indices constant

	City	# of pairs with $V(w_{ij})$ larger	# of pairs with $V(w_{ii^*})$ larger	Dixon & Mood test statistic	H0	H0 (Bonferroni)
1	Amsterdam	3.00	20	3.34	Reject	Reject
2	Bogota	1.00	32	5.22	Reject	Reject
3	Brussels	2.00	21	3.75	Reject	Reject
4	Buenos Aires	0.00	33	5.57	Reject	Reject
5	Caracas	0.00	33	5.57	Reject	Reject
6	Chicago	11.00	20	1.44	Do not reject	Do not reject
7	Copenhagen	0.00	23	4.59	Reject	Reject
8	Dublin	1.00	22	4.17	Reject	Reject
9	Geneva	2.00	30	4.77	Reject	Reject
10	Helsinki	0.00	23	4.59	Reject	Reject
11	Hong Kong	12.00	21	1.39	Do not reject	Do not reject
12	Johannesburg	2.00	31	4.87	Reject	Reject
13	Lisbon	2.00	21	3.75	Reject	Reject
14	London	5.00	28	3.83	Reject	Reject
15	Los Angeles	0.00	31	5.39	Reject	Reject
16	Luxembourg	1.00	22	4.17	Reject	Reject
17	Madrid	1.00	22	4.17	Reject	Reject
18	Manila	1.00	32	5.22	Reject	Reject
19	Mexico City	4.00	29	4.18	Reject	Reject
20	Milan	0.00	23	4.59	Reject	Reject
21	Montreal	5.00	27	3.71	Reject	Reject
22	New York	0.00	31	5.39	Reject	Reject
23	Oslo	9.00	24	2.44	Reject	Do not reject
24	Paris	3.00	20	3.34	Reject	Reject
25	Rio de Janeiro	0.00	32	5.48	Reject	Reject
26	Sao Paulo	0.00	32	5.48	Reject	Reject
27	Singapore	6.00	27	3.48	Reject	Reject
28	Stockholm	0.00	33	5.57	Reject	Reject
29	Sydney	0.00	33	5.57	Reject	Reject
30	Tel Aviv	10.00	23	2.09	Reject	Do not reject
31	Tokyo	0.00	33	5.57	Reject	Reject
32	Toronto	3.00	29	4.42	Reject	Reject
33	Vienna	7.00	16	1.67	Reject	Do not reject
34	Zurich	2.00	30	4.77	Reject	Reject

Table C.47: Dixon and Mood test based on MAD, for inter-country unskilled, vs skilled, 1976-2009 data while holding the composition of the indices constant

	City	# of pairs with $V(w_{ij})$ larger	# of pairs with $V(w_{ii^*})$ larger	Dixon & Mood test statistic	H0	H0 (Bonferroni)
1	Amsterdam	3.00	20	3.34	Reject	Reject
2	Bogota	1.00	32	5.22	Reject	Reject
3	Brussels	2.00	21	3.75	Reject	Reject
4	Buenos Aires	0.00	33	5.57	Reject	Reject
5	Caracas	0.00	33	5.57	Reject	Reject
6	Chicago	6.00	25	3.23	Reject	Reject
7	Copenhagen	0.00	23	4.59	Reject	Reject
8	Dublin	0.00	23	4.59	Reject	Reject
9	Geneva	2.00	30	4.77	Reject	Reject
10	Helsinki	0.00	23	4.59	Reject	Reject
11	Hong Kong	2.00	31	4.87	Reject	Reject
12	Johannesburg	0.00	33	5.57	Reject	Reject
13	Lisbon	3.00	20	3.34	Reject	Reject
14	London	6.00	27	3.48	Reject	Reject
15	Los Angeles	0.00	31	5.39	Reject	Reject
16	Luxembourg	0.00	23	4.59	Reject	Reject
17	Madrid	2.00	21	3.75	Reject	Reject
18	Manila	1.00	32	5.22	Reject	Reject
19	Mexico City	0.00	33	5.57	Reject	Reject
20	Milan	0.00	23	4.59	Reject	Reject
21	Montreal	3.00	29	4.42	Reject	Reject
22	New York	3.00	28	4.31	Reject	Reject
23	Oslo	6.00	27	3.48	Reject	Reject
24	Paris	4.00	19	2.92	Reject	Do not reject
25	Rio de Janeiro	6.00	26	3.36	Reject	Reject
26	Sao Paulo	0.00	32	5.48	Reject	Reject
27	Singapore	1.00	32	5.22	Reject	Reject
28	Stockholm	0.00	33	5.57	Reject	Reject
29	Sydney	0.00	33	5.57	Reject	Reject
30	Tel Aviv	3.00	30	4.53	Reject	Reject
31	Tokyo	0.00	33	5.57	Reject	Reject
32	Toronto	2.00	30	4.77	Reject	Reject
33	Vienna	8.00	15	1.25	Do not reject	Do not reject
34	Zurich	5.00	27	3.71	Reject	Reject

Table C.48: Dixon and Mood test based on Q^a_{adj}, for inter-country industry, vs services, 1976-2009 data while holding the composition of the indices constant

	City	# of pairs with $V(w_{ij})$ larger	# of pairs with $V(w_{ii^*})$ larger	Dixon & Mood test statistic	H0	H0 (Bonferroni)
1	Amsterdam	4.00	19	2.92	Reject	Do not reject
2	Bogota	7.00	26	3.13	Reject	Reject
3	Brussels	5.00	18	2.50	Reject	Do not reject
4	Buenos Aires	7.00	26	3.13	Reject	Reject
5	Caracas	23.00	10	NA	NA	NA
6	Chicago	0.00	31	5.39	Reject	Reject
7	Copenhagen	0.00	23	4.59	Reject	Reject
8	Dublin	1.00	22	4.17	Reject	Reject
9	Geneva	6.00	26	3.36	Reject	Reject
10	Helsinki	0.00	23	4.59	Reject	Reject
11	Hong Kong	22.00	11	NA	NA	NA
12	Johannesburg	2.00	31	4.87	Reject	Reject
13	Lisbon	13.00	10	NA	NA	NA
14	London	7.00	26	3.13	Reject	Reject
15	Los Angeles	1.00	30	5.03	Reject	Reject
16	Luxembourg	1.00	22	4.17	Reject	Reject
17	Madrid	0.00	23	4.59	Reject	Reject
18	Manila	1.00	32	5.22	Reject	Reject
19	Mexico City	0.00	33	5.57	Reject	Reject
20	Milan	0.00	23	4.59	Reject	Reject
21	Montreal	0.00	32	5.48	Reject	Reject
22	New York	0.00	31	5.39	Reject	Reject
23	Oslo	1.00	32	5.22	Reject	Reject
24	Paris	14.00	9	NA	NA	NA
25	Rio de Janeiro	0.00	32	5.48	Reject	Reject
26	Sao Paulo	1.00	31	5.13	Reject	Reject
27	Singapore	0.00	33	5.57	Reject	Reject
28	Stockholm	3.00	30	4.53	Reject	Reject
29	Sydney	1.00	32	5.22	Reject	Reject
30	Tel Aviv	19.00	14	NA	NA	NA
31	Tokyo	0.00	33	5.57	Reject	Reject
32	Toronto	0.00	32	5.48	Reject	Reject
33	Vienna	1.00	22	4.17	Reject	Reject
34	Zurich	1.00	31	5.13	Reject	Reject

Table C.49: Dixon and Mood test based on Q^a_{adj}, for inter-country services, vs industry, 1976-2009 data while holding the composition of the indices constant

	City	# of pairs with $V(w_{ij})$ larger	# of pairs with $V(w_{ii^*})$ larger	Dixon & Mood test statistic	H0	H0 (Bonferroni)
1	Amsterdam	5.00	18	2.50	Reject	Do not reject
2	Bogota	5.00	28	3.83	Reject	Reject
3	Brussels	4.00	19	2.92	Reject	Do not reject
4	Buenos Aires	5.00	28	3.83	Reject	Reject
5	Caracas	15.00	18	0.35	Do not reject	Do not reject
6	Chicago	4.00	27	3.95	Reject	Reject
7	Copenhagen	1.00	22	4.17	Reject	Reject
8	Dublin	2.00	21	3.75	Reject	Reject
9	Geneva	6.00	26	3.36	Reject	Reject
10	Helsinki	0.00	23	4.59	Reject	Reject
11	Hong Kong	31.00	2	NA	NA	NA
12	Johannesburg	2.00	31	4.87	Reject	Reject
13	Lisbon	15.00	8	NA	NA	NA
14	London	3.00	30	4.53	Reject	Reject
15	Los Angeles	5.00	26	3.59	Reject	Reject
16	Luxembourg	0.00	23	4.59	Reject	Reject
17	Madrid	3.00	20	3.34	Reject	Reject
18	Manila	1.00	32	5.22	Reject	Reject
19	Mexico City	2.00	31	4.87	Reject	Reject
20	Milan	0.00	23	4.59	Reject	Reject
21	Montreal	0.00	32	5.48	Reject	Reject
22	New York	0.00	31	5.39	Reject	Reject
23	Oslo	1.00	32	5.22	Reject	Reject
24	Paris	8.00	15	1.25	Do not reject	Do not reject
25	Rio de Janeiro	0.00	32	5.48	Reject	Reject
26	Sao Paulo	0.00	32	5.48	Reject	Reject
27	Singapore	0.00	33	5.57	Reject	Reject
28	Stockholm	2.00	31	4.87	Reject	Reject
29	Sydney	1.00	32	5.22	Reject	Reject
30	Tel Aviv	13.00	20	1.04	Do not reject	Do not reject
31	Tokyo	1.00	32	5.22	Reject	Reject
32	Toronto	1.00	31	5.13	Reject	Reject
33	Vienna	0.00	23	4.59	Reject	Reject
34	Zurich	0.00	32	5.48	Reject	Reject

Table C.50: Dixon and Mood test based on Q^a_{adj}, for inter-country competitive, vs uncompetitive, 1976-2009 data while holding the composition of the indices constant

	City	# of pairs with $V(w_{ij})$ larger	# of pairs with $V(w_{ii^*})$ larger	Dixon & Mood test statistic	H0	H0 (Bonferroni)
1	Amsterdam	1.00	22	4.17	Reject	Reject
2	Bogota	21.00	12	NA	NA	NA
3	Brussels	6.00	17	2.09	Reject	Do not reject
4	Buenos Aires	1.00	32	5.22	Reject	Reject
5	Caracas	7.00	26	3.13	Reject	Reject
6	Chicago	2.00	29	4.67	Reject	Reject
7	Copenhagen	0.00	23	4.59	Reject	Reject
8	Dublin	0.00	23	4.59	Reject	Reject
9	Geneva	10.00	22	1.94	Reject	Do not reject
10	Helsinki	0.00	23	4.59	Reject	Reject
11	Hong Kong	10.00	23	2.09	Reject	Do not reject
12	Johannesburg	0.00	33	5.57	Reject	Reject
13	Lisbon	8.00	15	1.25	Do not reject	Do not reject
14	London	1.00	32	5.22	Reject	Reject
15	Los Angeles	0.00	31	5.39	Reject	Reject
16	Luxembourg	0.00	23	4.59	Reject	Reject
17	Madrid	0.00	23	4.59	Reject	Reject
18	Manila	6.00	27	3.48	Reject	Reject
19	Mexico City	0.00	33	5.57	Reject	Reject
20	Milan	0.00	23	4.59	Reject	Reject
21	Montreal	4.00	28	4.07	Reject	Reject
22	New York	0.00	31	5.39	Reject	Reject
23	Oslo	13.00	20	1.04	Do not reject	Do not reject
24	Paris	7.00	16	1.67	Reject	Do not reject
25	Rio de Janeiro	0.00	32	5.48	Reject	Reject
26	Sao Paulo	0.00	32	5.48	Reject	Reject
27	Singapore	1.00	32	5.22	Reject	Reject
28	Stockholm	1.00	32	5.22	Reject	Reject
29	Sydney	0.00	33	5.57	Reject	Reject
30	Tel Aviv	0.00	33	5.57	Reject	Reject
31	Tokyo	0.00	33	5.57	Reject	Reject
32	Toronto	0.00	32	5.48	Reject	Reject
33	Vienna	1.00	22	4.17	Reject	Reject
34	Zurich	1.00	31	5.13	Reject	Reject

Table C.51: Dixon and Mood test based on Q^a_{adj}, for inter-country uncompetitive, vs competitive, 1976-2009 data while holding the composition of the indices constant

	City	# of pairs with $V(w_{ij})$ larger	# of pairs with $V(w_{ii^*})$ larger	Dixon & Mood test statistic	H0	H0 (Bonferroni)
1	Amsterdam	3.00	20	3.34	Reject	Reject
2	Bogota	25.00	8	NA	NA	NA
3	Brussels	9.00	14	0.83	Do not reject	Do not reject
4	Buenos Aires	2.00	31	4.87	Reject	Reject
5	Caracas	14.00	19	0.70	Do not reject	Do not reject
6	Chicago	15.00	16	0.00	Do not reject	Do not reject
7	Copenhagen	1.00	22	4.17	Reject	Reject
8	Dublin	1.00	22	4.17	Reject	Reject
9	Geneva	17.00	15	NA	NA	NA
10	Helsinki	0.00	23	4.59	Reject	Reject
11	Hong Kong	15.00	18	0.35	Do not reject	Do not reject
12	Johannesburg	0.00	33	5.57	Reject	Reject
13	Lisbon	15.00	8	NA	NA	NA
14	London	0.00	33	5.57	Reject	Reject
15	Los Angeles	2.00	29	4.67	Reject	Reject
16	Luxembourg	2.00	21	3.75	Reject	Reject
17	Madrid	2.00	21	3.75	Reject	Reject
18	Manila	15.00	18	0.35	Do not reject	Do not reject
19	Mexico City	4.00	29	4.18	Reject	Reject
20	Milan	1.00	22	4.17	Reject	Reject
21	Montreal	6.00	26	3.36	Reject	Reject
22	New York	0.00	31	5.39	Reject	Reject
23	Oslo	15.00	18	0.35	Do not reject	Do not reject
24	Paris	6.00	17	2.09	Reject	Do not reject
25	Rio de Janeiro	0.00	32	5.48	Reject	Reject
26	Sao Paulo	1.00	31	5.13	Reject	Reject
27	Singapore	2.00	31	4.87	Reject	Reject
28	Stockholm	1.00	32	5.22	Reject	Reject
29	Sydney	0.00	33	5.57	Reject	Reject
30	Tel Aviv	0.00	33	5.57	Reject	Reject
31	Tokyo	0.00	33	5.57	Reject	Reject
32	Toronto	0.00	32	5.48	Reject	Reject
33	Vienna	1.00	22	4.17	Reject	Reject
34	Zurich	3.00	29	4.42	Reject	Reject

Table C.52: Dixon and Mood test based on Q^a_{adj}, for inter-country skilled vs unskilled, 1976-2009 data while holding the composition of the indices constant

	City	# of pairs with $V(w_{ij})$ larger	# of pairs with $V(w_{ii^*})$ larger	Dixon & Mood test statistic	H0	H0 (Bonferroni)
1	Amsterdam	0.00	23	4.59	Reject	Reject
2	Bogota	2.00	31	4.87	Reject	Reject
3	Brussels	1.00	22	4.17	Reject	Reject
4	Buenos Aires	1.00	32	5.22	Reject	Reject
5	Caracas	31.00	2	NA	NA	NA
6	Chicago	2.00	29	4.67	Reject	Reject
7	Copenhagen	3.00	20	3.34	Reject	Reject
8	Dublin	1.00	22	4.17	Reject	Reject
9	Geneva	7.00	25	3.01	Reject	Reject
10	Helsinki	2.00	21	3.75	Reject	Reject
11	Hong Kong	3.00	30	4.53	Reject	Reject
12	Johannesburg	7.00	26	3.13	Reject	Reject
13	Lisbon	15.00	8	NA	NA	NA
14	London	6.00	27	3.48	Reject	Reject
15	Los Angeles	2.00	29	4.67	Reject	Reject
16	Luxembourg	2.00	21	3.75	Reject	Reject
17	Madrid	4.00	19	2.92	Reject	Do not reject
18	Manila	17.00	16	NA	NA	NA
19	Mexico City	0.00	33	5.57	Reject	Reject
20	Milan	1.00	22	4.17	Reject	Reject
21	Montreal	8.00	24	2.65	Reject	Do not reject
22	New York	1.00	30	5.03	Reject	Reject
23	Oslo	1.00	32	5.22	Reject	Reject
24	Paris	1.00	22	4.17	Reject	Reject
25	Rio de Janeiro	2.00	30	4.77	Reject	Reject
26	Sao Paulo	1.00	31	5.13	Reject	Reject
27	Singapore	9.00	24	2.44	Reject	Do not reject
28	Stockholm	1.00	32	5.22	Reject	Reject
29	Sydney	0.00	33	5.57	Reject	Reject
30	Tel Aviv	1.00	32	5.22	Reject	Reject
31	Tokyo	0.00	33	5.57	Reject	Reject
32	Toronto	2.00	30	4.77	Reject	Reject
33	Vienna	3.00	20	3.34	Reject	Reject
34	Zurich	3.00	29	4.42	Reject	Reject

Table C.53: Dixon and Mood test based on Q^a_{adj}, for inter-country unskilled vs skilled, 1976-2009 data while holding the composition of the indices constant

	City	# of pairs with $V(w_{ij})$ larger	# of pairs with $V(w_{ii^*})$ larger	Dixon & Mood test statistic	H0	H0 (Bonferroni)
1	Amsterdam	0.00	23	4.59	Reject	Reject
2	Bogota	2.00	31	4.87	Reject	Reject
3	Brussels	1.00	22	4.17	Reject	Reject
4	Buenos Aires	0.00	33	5.57	Reject	Reject
5	Caracas	27.00	6	NA	NA	NA
6	Chicago	1.00	30	5.03	Reject	Reject
7	Copenhagen	1.00	22	4.17	Reject	Reject
8	Dublin	0.00	23	4.59	Reject	Reject
9	Geneva	4.00	28	4.07	Reject	Reject
10	Helsinki	1.00	22	4.17	Reject	Reject
11	Hong Kong	3.00	30	4.53	Reject	Reject
12	Johannesburg	13.00	20	1.04	Do not reject	Do not reject
13	Lisbon	15.00	8	NA	NA	NA
14	London	3.00	30	4.53	Reject	Reject
15	Los Angeles	5.00	26	3.59	Reject	Reject
16	Luxembourg	2.00	21	3.75	Reject	Reject
17	Madrid	12.00	11	NA	NA	NA
18	Manila	16.00	17	0.00	Do not reject	Do not reject
19	Mexico City	1.00	32	5.22	Reject	Reject
20	Milan	0.00	23	4.59	Reject	Reject
21	Montreal	6.00	26	3.36	Reject	Reject
22	New York	5.00	26	3.59	Reject	Reject
23	Oslo	0.00	33	5.57	Reject	Reject
24	Paris	3.00	20	3.34	Reject	Reject
25	Rio de Janeiro	5.00	27	3.71	Reject	Reject
26	Sao Paulo	2.00	30	4.77	Reject	Reject
27	Singapore	7.00	26	3.13	Reject	Reject
28	Stockholm	1.00	32	5.22	Reject	Reject
29	Sydney	0.00	33	5.57	Reject	Reject
30	Tel Aviv	2.00	31	4.87	Reject	Reject
31	Tokyo	3.00	30	4.53	Reject	Reject
32	Toronto	4.00	28	4.07	Reject	Reject
33	Vienna	5.00	18	2.50	Reject	Do not reject
34	Zurich	4.00	28	4.07	Reject	Reject

Figure C.7: Distribution of ratios of inter-country to intra-country mean variances for profession level data corrected for city, year and profession fixed effects

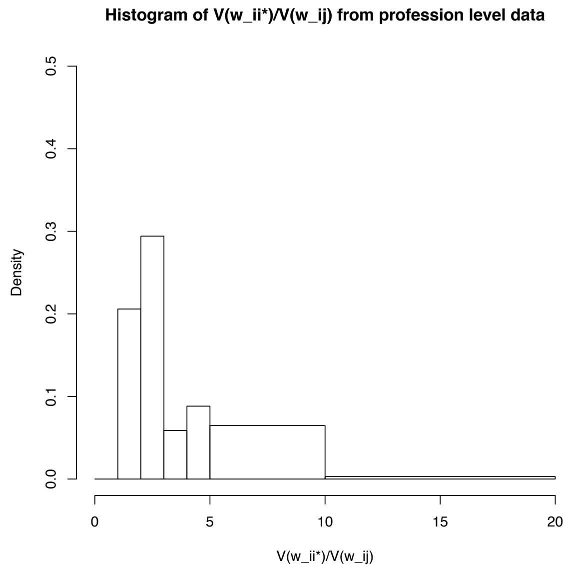


Table C.54: One-sided Levene test over all city pairs for inter-industry vs services

	City	# of pairs for which H0 not rejected	# of pairs for which H0 rejected	# of pairs for which H0 not rejected (Bonferroni)	# of pairs for which H0 rejected (Bonferroni)
1	Amsterdam	7	16	23	0
2	Bogota	29	4	33	0
3	Brussels	14	9	23	0
4	Buenos Aires	6	27	33	0
5	Caracas	0	33	33	0
6	Chicago	7	24	31	0
7	Copenhagen	6	17	23	0
8	Dublin	5	18	23	0
9	Geneva	20	12	31	1
10	Helsinki	4	19	22	1
11	Hong Kong	18	15	33	0
12	Johannesburg	27	6	33	0
13	Lisbon	11	12	23	0
14	London	13	20	31	2
15	Los Angeles	8	23	29	2
16	Luxembourg	9	14	23	0
17	Madrid	16	7	23	0
18	Manila	17	16	33	0
19	Mexico City	0	33	33	0
20	Milan	10	13	23	0
21	Montreal	4	28	30	2
22	New York	12	19	30	1
23	Oslo	11	22	30	3
24	Paris	15	8	23	0
25	Rio de Janeiro	14	18	32	0
26	Sao Paulo	16	16	32	0
27	Singapore	13	20	33	0
28	Stockholm	12	21	32	1
29	Sydney	5	28	32	1
30	Tel Aviv	11	22	33	0
31	Tokyo	12	21	33	0
32	Toronto	5	27	31	1
33	Vienna	12	11	23	0
34	Zurich	15	17	32	0

Table C.55: One-sided Levene test over all city pairs for inter-services vs industry

	City	# of pairs for which H0 not rejected	# of pairs for which H0 rejected	# of pairs for which H0 not rejected (Bonferroni)	# of pairs for which H0 rejected (Bonferroni)
1	Amsterdam	9	14	23	0
2	Bogota	31	2	33	0
3	Brussels	16	7	23	0
4	Buenos Aires	7	26	33	0
5	Caracas	1	32	33	0
6	Chicago	19	12	31	0
7	Copenhagen	6	17	23	0
8	Dublin	6	17	22	1
9	Geneva	19	13	30	2
10	Helsinki	3	20	21	2
11	Hong Kong	26	7	33	0
12	Johannesburg	18	15	33	0
13	Lisbon	12	11	23	0
14	London	14	19	30	3
15	Los Angeles	6	25	31	0
16	Luxembourg	17	6	23	0
17	Madrid	16	7	23	0
18	Manila	27	6	33	0
19	Mexico City	0	33	33	0
20	Milan	15	8	23	0
21	Montreal	7	25	31	1
22	New York	9	22	31	0
23	Oslo	16	17	32	1
24	Paris	13	10	23	0
25	Rio de Janeiro	11	21	32	0
26	Sao Paulo	22	10	32	0
27	Singapore	16	17	33	0
28	Stockholm	13	20	33	0
29	Sydney	8	25	33	0
30	Tel Aviv	22	11	33	0
31	Tokyo	14	19	33	0
32	Toronto	12	20	32	0
33	Vienna	14	9	23	0
34	Zurich	15	17	31	1

Table C.56: One-sided Levene test over all city pairs for inter-competitive vs uncompetitive

	City	# of pairs for which H0 not rejected	# of pairs for which H0 rejected	# of pairs for which H0 not rejected (Bonferroni)	# of pairs for which H0 rejected (Bonferroni)
1	Amsterdam	9	14	22	1
2	Bogota	30	3	32	1
3	Brussels	16	7	23	0
4	Buenos Aires	11	22	33	0
5	Caracas	0	33	33	0
6	Chicago	21	10	31	0
7	Copenhagen	12	11	23	0
8	Dublin	6	17	23	0
9	Geneva	22	10	31	1
10	Helsinki	10	13	22	1
11	Hong Kong	24	9	33	0
12	Johannesburg	28	5	33	0
13	Lisbon	16	7	23	0
14	London	10	23	33	0
15	Los Angeles	10	21	30	1
16	Luxembourg	15	8	23	0
17	Madrid	21	2	23	0
18	Manila	26	7	33	0
19	Mexico City	3	30	33	0
20	Milan	9	14	23	0
21	Montreal	13	19	31	1
22	New York	23	8	31	0
23	Oslo	27	6	33	0
24	Paris	18	5	23	0
25	Rio de Janeiro	10	22	32	0
26	Sao Paulo	8	24	32	0
27	Singapore	24	9	33	0
28	Stockholm	22	11	33	0
29	Sydney	14	19	32	1
30	Tel Aviv	3	30	33	0
31	Tokyo	17	16	33	0
32	Toronto	6	26	30	2
33	Vienna	13	10	23	0
34	Zurich	13	19	31	1

Table C.57: One-sided Levene test over all city pairs for inter-uncompetitive vs competitive

	City	# of pairs for which H0 not rejected	# of pairs for which H0 rejected	# of pairs for which H0 not rejected (Bonferroni)	# of pairs for which H0 rejected (Bonferroni)
1	Amsterdam	10	13	23	0
2	Bogota	29	4	33	0
3	Brussels	15	8	23	0
4	Buenos Aires	14	19	33	0
5	Caracas	1	32	33	0
6	Chicago	24	7	31	0
7	Copenhagen	12	11	23	0
8	Dublin	8	15	22	1
9	Geneva	19	13	32	0
10	Helsinki	6	17	22	1
11	Hong Kong	27	6	33	0
12	Johannesburg	18	15	33	0
13	Lisbon	18	5	23	0
14	London	12	21	30	3
15	Los Angeles	7	24	30	1
16	Luxembourg	17	6	23	0
17	Madrid	18	5	23	0
18	Manila	32	1	33	0
19	Mexico City	5	28	33	0
20	Milan	13	10	23	0
21	Montreal	13	19	32	0
22	New York	22	9	31	0
23	Oslo	25	8	33	0
24	Paris	17	6	23	0
25	Rio de Janeiro	11	21	32	0
26	Sao Paulo	20	12	32	0
27	Singapore	23	10	33	0
28	Stockholm	18	15	33	0
29	Sydney	9	24	33	0
30	Tel Aviv	16	17	33	0
31	Tokyo	10	23	33	0
32	Toronto	10	22	31	1
33	Vienna	14	9	23	0
34	Zurich	12	20	30	2

Table C.58: One-sided Levene test over all city pairs for inter-skilled vs unskilled

	City	# of pairs for which H0 not rejected	# of pairs for which H0 rejected	# of pairs for which H0 not rejected (Bonferroni)	# of pairs for which H0 rejected (Bonferroni)
1	Amsterdam	13	10	23	0
2	Bogota	30	3	33	0
3	Brussels	8	15	23	0
4	Buenos Aires	6	27	33	0
5	Caracas	7	26	33	0
6	Chicago	22	9	31	0
7	Copenhagen	3	20	22	1
8	Dublin	1	22	21	2
9	Geneva	16	16	32	0
10	Helsinki	8	15	22	1
11	Hong Kong	27	6	33	0
12	Johannesburg	31	2	33	0
13	Lisbon	2	21	23	0
14	London	20	13	32	1
15	Los Angeles	19	12	30	1
16	Luxembourg	13	10	23	0
17	Madrid	9	14	22	1
18	Manila	8	25	33	0
19	Mexico City	6	27	33	0
20	Milan	3	20	23	0
21	Montreal	20	12	32	0
22	New York	18	13	31	0
23	Oslo	19	14	33	0
24	Paris	16	7	23	0
25	Rio de Janeiro	21	11	32	0
26	Sao Paulo	25	7	32	0
27	Singapore	18	15	33	0
28	Stockholm	4	29	32	1
29	Sydney	7	26	33	0
30	Tel Aviv	26	7	33	0
31	Tokyo	3	30	32	1
32	Toronto	20	12	32	0
33	Vienna	7	16	22	1
34	Zurich	13	19	32	0

Table C.59: One-sided Levene test over all city pairs for inter-unskilled vs skilled

	City	# of pairs for which H0 not rejected	# of pairs for which H0 rejected	# of pairs for which H0 not rejected (Bonferroni)	# of pairs for which H0 rejected (Bonferroni)
1	Amsterdam	11	12	23	0
2	Bogota	29	4	33	0
3	Brussels	9	14	22	1
4	Buenos Aires	5	28	33	0
5	Caracas	10	23	33	0
6	Chicago	19	12	31	0
7	Copenhagen	5	18	20	3
8	Dublin	0	23	22	1
9	Geneva	14	18	31	1
10	Helsinki	7	16	22	1
11	Hong Kong	24	9	32	1
12	Johannesburg	25	8	33	0
13	Lisbon	3	20	23	0
14	London	20	13	32	1
15	Los Angeles	17	14	29	2
16	Luxembourg	13	10	23	0
17	Madrid	13	10	23	0
18	Manila	20	13	33	0
19	Mexico City	6	27	33	0
20	Milan	4	19	21	2
21	Montreal	19	13	32	0
22	New York	18	13	30	1
23	Oslo	14	19	32	1
24	Paris	15	8	23	0
25	Rio de Janeiro	29	3	32	0
26	Sao Paulo	30	2	32	0
27	Singapore	2	31	31	2
28	Stockholm	7	26	29	4
29	Sydney	6	27	32	1
30	Tel Aviv	29	4	33	0
31	Tokyo	4	29	32	1
32	Toronto	18	14	30	2
33	Vienna	7	16	22	1
34	Zurich	13	19	31	1

Table C.60: One-sided Fligner test over all city pairs for inter-industry vs services

	City	# of pairs for which H0 not rejected	# of pairs for which H0 rejected	# of pairs for which H0 not rejected (Bonferroni)	# of pairs for which H0 rejected (Bonferroni)
1	Amsterdam	9	14	23	0
2	Bogota	28	5	33	0
3	Brussels	12	11	23	0
4	Buenos Aires	6	27	33	0
5	Caracas	1	32	33	0
6	Chicago	9	22	31	0
7	Copenhagen	6	17	23	0
8	Dublin	7	16	23	0
9	Geneva	20	12	32	0
10	Helsinki	3	20	22	1
11	Hong Kong	20	13	33	0
12	Johannesburg	30	3	33	0
13	Lisbon	12	11	23	0
14	London	13	20	32	1
15	Los Angeles	10	21	31	0
16	Luxembourg	9	14	23	0
17	Madrid	15	8	23	0
18	Manila	16	17	33	0
19	Mexico City	0	33	33	0
20	Milan	9	14	23	0
21	Montreal	4	28	32	0
22	New York	11	20	31	0
23	Oslo	14	19	32	1
24	Paris	15	8	23	0
25	Rio de Janeiro	13	19	32	0
26	Sao Paulo	11	21	32	0
27	Singapore	15	18	33	0
28	Stockholm	17	16	32	1
29	Sydney	8	25	33	0
30	Tel Aviv	11	22	33	0
31	Tokyo	10	23	33	0
32	Toronto	8	24	32	0
33	Vienna	7	16	23	0
34	Zurich	14	18	32	0

Table C.61: One-sided Fligner test over all city pairs for inter-services vs industry

	City	# of pairs for which H0 not rejected	# of pairs for which H0 rejected	# of pairs for which H0 not rejected (Bonferroni)	# of pairs for which H0 rejected (Bonferroni)
1	Amsterdam	8	15	23	0
2	Bogota	30	3	33	0
3	Brussels	17	6	23	0
4	Buenos Aires	7	26	33	0
5	Caracas	1	32	33	0
6	Chicago	19	12	31	0
7	Copenhagen	6	17	23	0
8	Dublin	7	16	23	0
9	Geneva	19	13	32	0
10	Helsinki	2	21	23	0
11	Hong Kong	25	8	33	0
12	Johannesburg	19	14	33	0
13	Lisbon	14	9	23	0
14	London	15	18	32	1
15	Los Angeles	10	21	31	0
16	Luxembourg	16	7	23	0
17	Madrid	15	8	23	0
18	Manila	28	5	33	0
19	Mexico City	0	33	33	0
20	Milan	14	9	23	0
21	Montreal	7	25	32	0
22	New York	7	24	31	0
23	Oslo	16	17	33	0
24	Paris	14	9	23	0
25	Rio de Janeiro	5	27	32	0
26	Sao Paulo	21	11	32	0
27	Singapore	16	17	33	0
28	Stockholm	13	20	33	0
29	Sydney	9	24	33	0
30	Tel Aviv	20	13	33	0
31	Tokyo	12	21	33	0
32	Toronto	12	20	32	0
33	Vienna	11	12	23	0
34	Zurich	13	19	32	0

Table C.62: One-sided Fligner test over all city pairs for inter-competitive vs uncompetitive

	City	# of pairs for which H0 not rejected	# of pairs for which H0 rejected	# of pairs for which H0 not rejected (Bonferroni)	# of pairs for which H0 rejected (Bonferroni)
1	Amsterdam	11	12	23	0
2	Bogota	13	20	33	0
3	Brussels	16	7	23	0
4	Buenos Aires	15	18	33	0
5	Caracas	1	32	33	0
6	Chicago	21	10	31	0
7	Copenhagen	3	20	23	0
8	Dublin	8	15	23	0
9	Geneva	20	12	32	0
10	Helsinki	11	12	23	0
11	Hong Kong	24	9	33	0
12	Johannesburg	28	5	33	0
13	Lisbon	15	8	23	0
14	London	11	22	32	1
15	Los Angeles	9	22	31	0
16	Luxembourg	13	10	23	0
17	Madrid	19	4	23	0
18	Manila	22	11	33	0
19	Mexico City	8	25	33	0
20	Milan	7	16	23	0
21	Montreal	10	22	32	0
22	New York	20	11	31	0
23	Oslo	25	8	33	0
24	Paris	18	5	23	0
25	Rio de Janeiro	9	23	32	0
26	Sao Paulo	12	20	32	0
27	Singapore	23	10	33	0
28	Stockholm	22	11	33	0
29	Sydney	14	19	33	0
30	Tel Aviv	3	30	33	0
31	Tokyo	3	30	33	0
32	Toronto	7	25	32	0
33	Vienna	9	14	23	0
34	Zurich	12	20	32	0

Table C.63: One-sided Fligner test over all city pairs for inter-uncompetitive vs competitive

	City	# of pairs for which H0 not rejected	# of pairs for which H0 rejected	# of pairs for which H0 not rejected (Bonferroni)	# of pairs for which H0 rejected (Bonferroni)
1	Amsterdam	10	13	23	0
2	Bogota	12	21	33	0
3	Brussels	16	7	23	0
4	Buenos Aires	16	17	33	0
5	Caracas	1	32	33	0
6	Chicago	25	6	31	0
7	Copenhagen	1	22	23	0
8	Dublin	11	12	23	0
9	Geneva	21	11	32	0
10	Helsinki	8	15	23	0
11	Hong Kong	25	8	33	0
12	Johannesburg	22	11	33	0
13	Lisbon	16	7	23	0
14	London	14	19	32	1
15	Los Angeles	5	26	31	0
16	Luxembourg	16	7	23	0
17	Madrid	15	8	23	0
18	Manila	32	1	33	0
19	Mexico City	9	24	33	0
20	Milan	6	17	23	0
21	Montreal	11	21	32	0
22	New York	14	17	31	0
23	Oslo	26	7	33	0
24	Paris	17	6	23	0
25	Rio de Janeiro	12	20	32	0
26	Sao Paulo	20	12	32	0
27	Singapore	23	10	33	0
28	Stockholm	17	16	33	0
29	Sydney	11	22	33	0
30	Tel Aviv	16	17	33	0
31	Tokyo	0	33	33	0
32	Toronto	12	20	32	0
33	Vienna	5	18	23	0
34	Zurich	10	22	32	0

Table C.64: One-sided Fligner test over all city pairs for inter-skilled vs unskilled

	City	# of pairs for which H0 not rejected	# of pairs for which H0 rejected	# of pairs for which H0 not rejected (Bonferroni)	# of pairs for which H0 rejected (Bonferroni)
1	Amsterdam	13	10	23	0
2	Bogota	30	3	33	0
3	Brussels	8	15	23	0
4	Buenos Aires	9	24	33	0
5	Caracas	8	25	33	0
6	Chicago	23	8	31	0
7	Copenhagen	3	20	23	0
8	Dublin	2	21	23	0
9	Geneva	12	20	32	0
10	Helsinki	8	15	23	0
11	Hong Kong	26	7	33	0
12	Johannesburg	31	2	33	0
13	Lisbon	4	19	23	0
14	London	22	11	32	1
15	Los Angeles	18	13	31	0
16	Luxembourg	11	12	23	0
17	Madrid	10	13	23	0
18	Manila	13	20	33	0
19	Mexico City	9	24	33	0
20	Milan	2	21	23	0
21	Montreal	21	11	32	0
22	New York	16	15	31	0
23	Oslo	20	13	33	0
24	Paris	16	7	23	0
25	Rio de Janeiro	19	13	32	0
26	Sao Paulo	25	7	32	0
27	Singapore	20	13	33	0
28	Stockholm	6	27	33	0
29	Sydney	8	25	33	0
30	Tel Aviv	26	7	33	0
31	Tokyo	5	28	33	0
32	Toronto	19	13	32	0
33	Vienna	6	17	23	0
34	Zurich	10	22	32	0

Table C.65: One-sided Fligner test over all city pairs for inter-unskilled vs skilled

	City	# of pairs for which H0 not rejected	# of pairs for which H0 rejected	# of pairs for which H0 not rejected (Bonferroni)	# of pairs for which H0 rejected (Bonferroni)
1	Amsterdam	11	12	23	0
2	Bogota	29	4	33	0
3	Brussels	11	12	22	1
4	Buenos Aires	9	24	33	0
5	Caracas	11	22	33	0
6	Chicago	19	12	31	0
7	Copenhagen	4	19	22	1
8	Dublin	1	22	22	1
9	Geneva	14	18	32	0
10	Helsinki	7	16	23	0
11	Hong Kong	25	8	33	0
12	Johannesburg	24	9	33	0
13	Lisbon	5	18	23	0
14	London	21	12	32	1
15	Los Angeles	17	14	31	0
16	Luxembourg	9	14	23	0
17	Madrid	15	8	23	0
18	Manila	21	12	33	0
19	Mexico City	7	26	33	0
20	Milan	1	22	23	0
21	Montreal	19	13	32	0
22	New York	17	14	31	0
23	Oslo	17	16	33	0
24	Paris	18	5	23	0
25	Rio de Janeiro	29	3	32	0
26	Sao Paulo	29	3	32	0
27	Singapore	4	29	33	0
28	Stockholm	7	26	32	1
29	Sydney	3	30	33	0
30	Tel Aviv	28	5	33	0
31	Tokyo	5	28	32	1
32	Toronto	18	14	32	0
33	Vienna	6	17	23	0
34	Zurich	13	19	31	1

Figure C.8: Distribution of ratios of inter-country to intra-country mean variances for profession level data corrected for city, year and profession fixed effects and city-year interaction terms

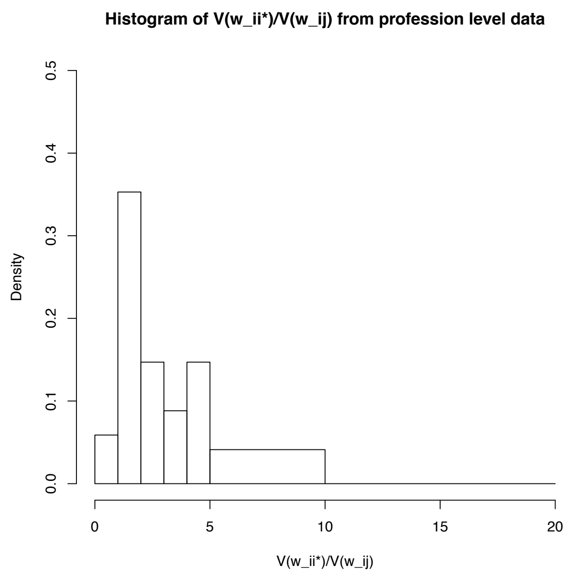


Table C.66: Comparison of variance of inter- and intra-country wage differentials after correcting for city, year and profession fixed effects

	City	Inter-county mean variance	Intra-country mean variance	Inter larger?	Ratio of mean variances
1	Amsterdam	0.15	0.02	TRUE	7.18
2	Bogota	0.22	0.19	TRUE	1.16
3	Brussels	0.17	0.03	TRUE	5.62
4	Buenos Aires	0.31	0.18	TRUE	1.72
5	Caracas	0.75	0.29	TRUE	2.57
6	Chicago	0.14	0.04	TRUE	3.46
7	Copenhagen	0.16	0.02	TRUE	9.94
8	Dublin	0.22	0.02	TRUE	8.91
9	Geneva	0.13	0.02	TRUE	5.76
10	Helsinki	0.17	0.02	TRUE	11.30
11	Hong Kong	0.27	0.23	TRUE	1.17
12	Johannesburg	0.22	0.10	TRUE	2.14
13	Lisbon	0.24	0.10	TRUE	2.45
14	London	0.16	0.03	TRUE	4.97
15	Los Angeles	0.13	0.06	TRUE	2.39
16	Luxembourg	0.17	0.03	TRUE	6.47
17	Madrid	0.19	0.07	TRUE	2.73
18	Manila	0.19	0.14	TRUE	1.40
19	Mexico City	0.50	0.19	TRUE	2.60
20	Milan	0.20	0.03	TRUE	5.83
21	Montreal	0.14	0.05	TRUE	2.85
22	New York	0.14	0.05	TRUE	2.98
23	Oslo	0.13	0.03	TRUE	4.32
24	Paris	0.16	0.07	TRUE	2.45
25	Rio de Janeiro	0.29	0.17	TRUE	1.75
26	Sao Paulo	0.27	0.16	TRUE	1.67
27	Singapore	0.20	0.09	TRUE	2.36
28	Stockholm	0.13	0.01	TRUE	8.62
29	Sydney	0.13	0.02	TRUE	6.04
30	Tel Aviv	0.17	0.09	TRUE	1.92
31	Tokyo	0.18	0.04	TRUE	4.49
32	Toronto	0.13	0.04	TRUE	3.02
33	Vienna	0.17	0.02	TRUE	7.86
34	Zurich	0.14	0.01	TRUE	9.60

Table C.67: Comparison of variance of inter- and intra-country wage differentials after correcting for city, year and profession fixed effects, and city-year interaction terms

	City	Inter-county mean variance	Intra-country mean variance	Inter larger?	Ratio of mean variances
1	Amsterdam	0.12	0.02	TRUE	5.71
2	Bogota	0.19	0.19	FALSE	1.00
3	Brussels	0.13	0.03	TRUE	4.33
4	Buenos Aires	0.28	0.18	TRUE	1.58
5	Caracas	0.47	0.29	TRUE	1.62
6	Chicago	0.11	0.04	TRUE	2.71
7	Copenhagen	0.12	0.02	TRUE	7.57
8	Dublin	0.12	0.02	TRUE	5.02
9	Geneva	0.10	0.02	TRUE	4.43
10	Helsinki	0.13	0.02	TRUE	8.62
11	Hong Kong	0.21	0.23	FALSE	0.93
12	Johannesburg	0.13	0.10	TRUE	1.31
13	Lisbon	0.18	0.10	TRUE	1.84
14	London	0.11	0.03	TRUE	3.25
15	Los Angeles	0.11	0.06	TRUE	1.99
16	Luxembourg	0.13	0.03	TRUE	4.73
17	Madrid	0.14	0.07	TRUE	2.05
18	Manila	0.16	0.14	TRUE	1.14
19	Mexico City	0.32	0.19	TRUE	1.70
20	Milan	0.15	0.03	TRUE	4.51
21	Montreal	0.11	0.05	TRUE	2.12
22	New York	0.11	0.05	TRUE	2.38
23	Oslo	0.10	0.03	TRUE	3.33
24	Paris	0.12	0.07	TRUE	1.84
25	Rio de Janeiro	0.24	0.17	TRUE	1.48
26	Sao Paulo	0.23	0.16	TRUE	1.43
27	Singapore	0.14	0.09	TRUE	1.62
28	Stockholm	0.10	0.01	TRUE	6.54
29	Sydney	0.10	0.02	TRUE	4.69
30	Tel Aviv	0.13	0.09	TRUE	1.51
31	Tokyo	0.13	0.04	TRUE	3.33
32	Toronto	0.10	0.04	TRUE	2.33
33	Vienna	0.12	0.02	TRUE	5.68
34	Zurich	0.10	0.01	TRUE	7.17

Appendix D

Appendix for Chapter 3

Table D.1: Difference-in-Differences regression results for skill ratio L3/L1 using the synthetic control as the untreated control group

	Estimate	Std. Error	t value	Pr(>|t|)
(Intercept)	2.5567	0.3555	7.1918	0.0000
Treatment	-0.0083	0.5028	-0.0166	0.9870
Period	-0.2409	0.4590	-0.5248	0.6069
Treatment:Period	1.1285	0.6491	1.7387	0.1013

Table D.2: Difference-in-Differences regression results for skill ratio L3/L1 using all other cities as the untreated control group

	Estimate	Std. Error	t value	Pr(>|t|)
(Intercept)	2.7624	0.1082	25.5300	0.0000
Treatment	-0.2140	0.6492	-0.3297	0.7418
Period	-0.0754	0.1397	-0.5397	0.5897
Treatment:Period	0.9630	0.8381	1.1490	0.2513

Table D.3: Difference-in-Differences regression results for skill ratio L3/L2 using the synthetic control as the untreated control group

	Estimate	Std. Error	t value	Pr(>|t|)
(Intercept)	1.4035	0.1142	12.2868	0.0000
Treatment	-0.0050	0.1615	-0.0308	0.9758
Period	0.0793	0.1475	0.5375	0.5983
Treatment:Period	0.3734	0.2085	1.7903	0.0923

Table D.4: Difference-in-Differences regression results for skill ratio L3/L2 using all other cities as the untreated control group

	Estimate	Std. Error	t value	Pr(>|t|)
(Intercept)	1.6893	0.0302	55.8558	0.0000
Treatment	-0.2908	0.1815	-1.6026	0.1099
Period	-0.0244	0.0390	-0.6244	0.5328
Treatment:Period	0.4770	0.2343	2.0362	0.0425

Table D.5: Difference-in-Differences regression results for skill ratio L2/L1 using the synthetic control as the untreated control group, 1982 to 2003

	Estimate	Std. Error	t value	Pr(>|t|)
(Intercept)	1.8637	0.1629	11.4414	0.0000
Treatment	-0.0130	0.2304	-0.0566	0.9558
Period	0.2350	0.2304	1.0200	0.3278
Treatment:Period	-0.5406	0.3258	-1.6593	0.1229

Table D.6: Difference-in-Differences regression results for skill ratio L2/L1 using the synthetic control as the untreated control group, post-treatment 2006-2009

	Estimate	Std. Error	t value	Pr(>|t|)
(Intercept)	1.8637	0.1856	10.0434	0.0000
Treatment	-0.0130	0.2624	-0.0497	0.9616
Period	-0.2282	0.3214	-0.7101	0.4978
Treatment:Period	0.8186	0.4545	1.8008	0.1094

Table D.7: Difference-in-Differences regression results for skill ratio L2/L1 using all other cities as the untreated control group, 1982 to 2003

	Estimate	Std. Error	t value	Pr(>|t|)
(Intercept)	1.6212	0.0513	31.6154	0.0000
Treatment	0.2295	0.3077	0.7460	0.4563
Period	0.0168	0.0725	0.2323	0.8165
Treatment:Period	-0.3224	0.4351	-0.7411	0.4593

Table D.8: Difference-in-Differences regression results for skill ratio L2/L1 using all other cities as the untreated control group, post-treatment 2006-2009

	Estimate	Std. Error	t value	Pr(>|t|)
(Intercept)	1.6212	0.0502	32.2772	0.0000
Treatment	0.2295	0.3014	0.7616	0.4472
Period	-0.0987	0.0870	-1.1349	0.2577
Treatment:Period	0.6890	0.5220	1.3201	0.1882

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